Philosophy of Science

VOL. 6 July, 1939 NO. 3

CONTENTS

What Is an Atom? William Marias Malisoff - - 261
A Materialistic Interpretation of Life. William Seifriz - - - - - - - - - - - - - - 266
Is Nature Rational? Ralph B. Winn - - - - 285
Physics, A Vicious Abstraction. Horace S. Fries - 301
Our Knowledge of Other Minds. C. D. Hardie - 309
Science: Natural and Social. Thomas I. Cook - 318
The Place of Theory in Anthropological Studies. Clyde Kluckhohn - - - - - - - - - - 328
Research Procedure and Laws of Culture. Alexander Lesser - - - - - - - - - - - - 345
Natural Languages as Cultural Indices. W. H. Werkmeister - - - - - - - - - - - - 356
Discussion (Correspondence) - - - - - - - - 367
Reviews and Notes - - - - - - - - - - - - 378

Copyright 1939, The Williams & Wilkins Company

Topics Treated in the Book

- First steps in treating scientific data
- Putting your ideas on paper
  - Plan and order of topics
- Outline of a thesis
- General suggestions on subject matter and arrangement
  - Title: Discussion of Results: Summary
- Outlines for various compositions
- Use of tenses in scientific papers
- Revisions
- Preparation of typewritten copy
- Corrections and alterations in manuscript for printer
  - Corrections
  - Insertions
  - Transpositions
  - Renumbering pages
  - Final Revisions
- Kinds of type and their indication in manuscript
  - Capitals
  - Italics
  - Numbers
- Abbreviation of units of weight and measure
  - A set of standard abbreviations
  - Abbreviations of the American Institute of Electrical Engineers
- Names of plants and animals
- Tables
- Footnotes
- Literature citations
- Abstracts and quotations
- Acknowledgments
- Preparation of analytical table of contents
- Headings in the text of an article
- Proofreading
  - Proofreaders' marks
  - Method of proofreading
  - Miscellaneous suggestions
  - Page proof
- Illustrations
  - Drawings
  - Graphs
  - Photographs
  - Preparation of copy
- Bibliography

PREPARATION *of* SCIENTIFIC *and* TECHNICAL PAPERS

By SAM F. TRELEASE

Assistant Professor of Botany at Columbia University

and EMMA SAREPTA YULE

Associate Professor of English in the University of the Philippines

A Real Help in Every Stage of the Preparation of a Manuscript

It appears to be the simplest thing in the world to set down ideas on paper, and to have what is set down appear in print.

But as a matter of fact there are dozens of complications, which are often not fully realized until the attempt is made, until author and publisher have become thoroughly tried in their endeavor to procure an understanding of each other.

An author frequently blames a publisher for faults in proof or in finished book that can be directly traced to faulty "copy." "You should have known what I intended" is an inadequate retort in such cases.

For the first time a brief concise text has been prepared for general distribution which sets forth certain standards which, if author and publisher both refer to it, will eliminate a deal of useless friction.

No author of scientific literature can fail to receive real help from a study of this concise manual. It not only makes his task of writing easier, but it enables him to get his material into the printed dress in which he desires to see it.

Third Edition Revised and Enlarged. ***$1.50***

PUBLISHED BY

THE WILLIAMS & WILKINS COMPANY

Publishers of Scientific Books and Periodicals

BALTIMORE, U. S. A.

Philosophy of Science

VOL. 6 July, 1939 NO. 3

What Is an Atom?

BY

WILLIAM MARIAS MALISOFF

FOR a number of years I have been demonstrating at my seminars that the atom is no longer atomic, that mechanics is no longer mechanistic, and that similar paradoxes of advancing science can be formulated throughout its extent. The point has been reached, however, that a call for a revision of terms or some such reform should be issued. At one time, indeed, I gave a list of terms to be dropped altogether. They refused to be dropped. I ask now: Will they permit themselves to be modified or to be reallocated?

The term "atom" is chosen for discussion first, because it will probably resist change more than any other. Its rivals have received the name of "ultimate particles". Their special names, e.g. "proton", permit a revision, should a structure be discovered in them, by the mere statement that they were mistaken for ultimate particles. The atom, on the contrary, was found to be not an atom (an indivisible) but is still called the atom. Everyone now knows that there are such things as "atomic structure", "atomic fragments" and the like. Is there any use, then, in trying to return the term to its original meaning? Should one labor on alternative names as "sub-molecule" or "elementon"? Or should one make the best of it by retaining the well-entrenched name, but guarding against misunderstanding by making special distinctions.

The greatest hope at the moment, it seems, lies in making a distinction which will put one on guard against the greatest number of misconceptions that have arisen with regard to the atom. The distinction I would favor is between *atomistic* and *atomic*. "Atomic" will mean "pertaining to the atom", "atomistic" will mean "pertaining to a reducibility to something indivisible".

We would thus be enabled by this adjustment of vocabulary to propose such generalizations as that "all scientific systems are subject to an atomistic analysis", meaning thereby that when properly analyzed they can all show something which will play the rôle of an indivisible (relative to that analysis). The "atomistic" analysis will lead either to electrons or quanta or simple ideas or sensations or, as we shall see, entities of any magnitude provided they are themselves taken as structureless.

The above proposal could be made to cover cases where the atom (considered as a structure in a *different* context) could still be a genuine atom in the sense of being the indivisible of an atomistic analysis. Thus old, valuable portions of science can be conserved in their sphere of usefulness without being "wrong". An instance of this is the kinetic gas theory for an ideal monatomic gas. The atoms of such a gas are indivisible within the scope of or for the purpose of this type of atomistic analysis. Note that the statement was made about an "ideal" monatomic gas. Thereby we ruled out the structure of the atom from consideration, and have thus kept it a genuine atom for the scope or context of the discussion.

We can then answer the questions: When is an atom an atom, and when is it not an atom? It is an atom only when it is the terminus of an atomistic analysis.

To practice the use of the term "atomistic" one might venture to state: an atomistic analysis is always possible. A problem might be set by the question: are there alternative or equivalent atomistic analyses? An extension of meaning can be suggested by the proposal: atomistic analyses need not deal with particles only, but may yield "logical atoms", "atomic propositions", "Freudian individuals", "souls", "points", "platonic ideas", "essences", and other relative indivisibles.

I have had to take note of the term "structure" along with the term "atom". An atomistic analysis is the analysis of a structure. From the viewpoint of that analysis its atom has no structure. The analysis is the total situation and it factors into two abstractions:—the atom (structureless) and the structure (non atom). I greatly favor the use of a hyphen when such an issue has to be stressed. To prevent misunderstanding, then, one should speak of *atomistic-structural* analysis. Dialectical materialists have a longer and probably an equally effective way of indicating the same idea. A dialectical materialist would say: There is no structure without atoms, there are no atoms without structure.

I call pairs, like the above, *conjugates*. The term is suggested both by the marital connotations and by mathematics which obtains a "real" number as the product of conjugate complex numbers. When someone produces a philosophy based exclusively on one member of such a wedded pair, it is high time to call in the other member, and to invoke the hyphen as the sign of an indissoluble marriage.

It looks as if we shall have atoms with us as long as we recognize structures. They are "the other". Those who like formulas may write: The material = structure $\times$ atomicity, or $M = S\,A$. There are, of course, other ways of factoring the material or "matter". You may use such a formula as one of the ways of defining the material and thus call yourself materialist in an enriched sense.

There is one structure of eminent importance in connection with the atom. It is the structure we might call "an observer observing an atom". The simplest observer (if we may strain the term) would be another atom, a structureless observer. The observer may well be an instrument, a complicated machine, a conscious human being. For the purposes of mathematical physics, anyhow, some claim it is not necessary to go all the way to a conscious human being. Be that as it may, what we call "observer" or "observing system" forms a structure with what is observed; and what is observed, if it is treated in a certain way, is an atom. It is an atom when the interaction with the observer does not raise the question of *its* structure. Thus, if

it is a volume which remains undissected, it is all surface. The atom is all face. Presumably even the conscious observer from the viewpoint of the simplest interaction is also all face. There you have, then, two atoms *facing* one another. All this I am bringing up because I believe that the existence of observers thus drags in the existence of atoms. Furthermore, I believe that it is inevitable on this view that when you "eliminate the observer", you thereby "eliminate the atom". I think that systems of thought which remove or eliminate any *particular* observer will fail to locate any *particular* atom. Some such dilemma may be lurking in relativity theory which cannot find atoms (electrons) of decent identifiable localizability in space-time.

The universe-as-a-whole is another such atom in hopeless search of its structure (a structure of which it would be an indivisible part). If, on the other hand, we speak of the "structure of the universe" we are using an expression like the "structure of the atom" and we have no true atom at all. The "universe-as-a whole" is a bachelor term. It is "vacuously satisfied", as the mathematicians say, and can acquire no material character since it has no mate. For the scientist this is tantamount to having no meaning. We can merely speak of the universe in this sense as an ever receding ideal, a Grenzbegriff which can never be quite reached. It means no more than any other infinitely regressive character.

The same structure may well apply to something one might want to call "the ultimate atom", meaning thereby the *last* of a series of efforts to apply atomistic-structural analysis to matter (in our liberal sense). There need be no last in such a series. Science does not need *actual* infinitesimals any more than it needs *actual* infinites. It can and does use the infinitesimals and infinites of variables approaching limits, characteristic of the calculus, but not implying any *actual* states of affairs.

The atom is as inaccessible as *the* universe. In a relative sense there are innumerable atoms and universes and they are all accessible.

So far I have said nothing about the difficulty of observing atoms (in the traditional sense) which has so fascinated physicists

and laymen. The reason is that I see no difficulty. I have described the atom by its character of being a structureless something within a structure, and find that size or magnitude is not involved at all. A tree can be an atom in an atomistic analysis of a forest, so long as its structure is not in question, i.e. its structure is non-existent in the context of the given analysis. This, incidentally, takes the wind out of the sails of the navigators of this theoretical sea, who propose to distinguish an atom from a tree, on the basis of the tree being a simple perception and the atom being a "construct". Some atoms may be too large to be seen. It is the difficulty of the demonstration of the *meaning* that varies. It is easy to *point* to a tree, it requires much more pointing to point out a chemical atom. The difficulty, however, has nothing to do with the character of being an atom in a given analysis.

I may be challenged concerning the *static* nature of my discussion of matter as factored into structure and atomicity. I am quite willing to give it all motion, so that we have a more complete conjugation as is shown in the hyphenated expression, *matter-in-motion*. Both the atomicity and the structure can receive dynamic quality simultaneously. It will be found that it is not necessarily the atom alone that is moving but the structure as well. I do not hope at this point to solve the problem of giving motion to a logical analysis or of kaleidoscoping a series of logical analyses or of showing how histories can be constructed for atoms-in-structure. I merely want to note that I have not forgotten the problem of motion and to point out that it need not be dumped entirely on the atoms. If only atoms move we are likely to get extravaganzas of "pure" mechanism. If the structures (configurations, patterns) move we can get away from naive mechanics. The atom is not committed to only one kind of mechanics or to mechanics only. Its behavior along with its structure need not be mechanistic. Systems which fully illustrate matter-in-motion, I call by contrast "materialistic". The fully materialistic atom with its structure must be dynamic.

Polytechnic Institute, Brooklyn, New York.

A Materialistic Interpretation of Life

BY

WILLIAM SEIFRIZ

THE attribute of living is due either to a specific state of matter or to a distinctive kind of energy. All possible interpretations of these two concepts are materialistic with one exception. There is said to be a kind of energy apart from this world. Such an assumption is vitalistic.

It is the purpose of the present thesis to show that energy is secondary to structure, and that life owes its existence to a specific state of matter.

Attempts to bridge the gap between materialism and vitalism, to soften the harshness of the one and strengthen the emotionalism of the other, have led to various speculations on the nature of life. Some philosophers have postulated an "organic-determinism", others, an "emergent evolution", and still others have looked "beyond" mechanism and vitalism. Such theories, though thought provoking, still leave but the two original possibilities, namely, materialism and vitalism, for all postulates, other than the purely vitalistic one, can be handled materialistically.

Functioning systems consist of a mechanism and a source of energy. In the case of a watch, the mechanism is the coordinated aggregate of sprockets; the energy lies in the wound spring. Valves, piston, and Stevenson link are units in the mechanism

of a locomotive; the energy is in the expanding steam. There is no reason why the same situation should not exist in the living mechanism.

Structure and energy determine function and are, therefore, indissociable in any dynamic system. This is particularly true of protoplasm, for life is above all dynamic, not static. However, a clock is not less a clock when, with wound spring, its pendulum hangs idle; nor is a locomotive less a mechanism for hauling cars when it stands idle upon a track. The potentialities of time-keeping or of traction are present even though the mechanism is not functioning. So may it be with protoplasm, the basic living substance. One of the purposes of this thesis is to lend support to the view that none of the processes commonly attributed to living matter need be present and the substance yet remain alive in the sense that it still possesses the potentialities of life. In short, although life in its fullest meaning is an aggregation of processes, there may be times, as during periods of prolonged dormancy, when protoplasm is in a state of complete rest, the potentialities but not the processes of life being present. Such a situation would mean that structure and not energy is the distinguishing feature of life.

The selection of a basis of classification is a personal prerogative. Structure, energy, or function may be chosen as the essential attribute of any class of mechanisms. Energy is unquestionably significant in functioning mechanisms. Indeed, it will later be shown that the essential attribute of life is the capacity of living matter to use energy in the synthesis of protoplasm, whereas in the syntheses of non-living organic substances energy is liberated. But it will also be shown that the capacity to use energy is a property acquired through a specific arrangement of parts, that is to say, through structure. An analogy will help clarify this point of view.

The class "locomotives" may be distinguished from the class "timepieces" on the basis of the kind of energy used, steam for locomotives and springs under tension for timepieces. But locomotives and timepieces operate equally well on other sources of energy; in fact, both may be operated on the same kind of

energy and thus become of one energy class though of distinct structural and functional classes. Structure and function, not energy, distinguish electric clocks from electric locomotives, and this may also be true of the two classes, living and non-living matter.

Before considering the contribution to be made here, namely, that a specific structure elevates living matter above the non-living, it will be well to dispose briefly of those other qualities which have long been regarded as possessed only by organisms, and are, therefore, distinctive of them alone.

The tenet, that life owes its existence to a *spiritus vitae* has always been the basic concept of religious and certain other forms of philosophic reasoning. A vital force is beyond the world of physics and chemistry, and consequently cannot be dealt with. The total energy of a system is the sum of all forms of energy present, but as vital energy cannot be computed it does not appear in the final sum. This serves a good purpose in religion, for vital energy is enigmatical, and therefore better suited to enshroud life in a veil of mystery.

But vitalism had another bulwark of defense, namely, that only living beings are capable of synthesizing organic matter. This assumption collapsed a century ago when urea was artificially made, thus proving that the capacity to synthesize complex carbon compounds is not characteristic of life alone. Since then, sugars, protein derivatives, porphyrins, and thyroxin have been made in the laboratory, so that there now appears to be no limit to what man may synthesize, unless it be protoplasm itself.

Growth, reproduction, respiration, and like functions have become traditional distinguishing features of life, yet each, when analyzed, is seen to have its counterpart in the non-living world.

Growth is not a distinguishing feature of life, for crystals grow, and in a manner so much like the symmetrical growth of plants and animals that biologists have speculated on the possible bearing of crystal growth on the symmetry of organisms.

Reproduction is characteristic of living beings, but it has its counterpart in non-living systems. A droplet of mercury may be made to pinch in half spontaneously and thus produce two of

its kind, and in a manner very much like that of bacteria and protozoa.

Analogies often mislead, yet when honestly presented equally often clarify. It is maintained that the growth or reproduction of inanimate objects is quite a different thing from that of living organisms. Attempts have, therefore, been made to distinguish more sharply between the growth of non-living systems and that of the living system by differentiating between exogenous growth, or growth from without, as in crystals, and endogenous growth or growth from within, as in organisms. The distinction does not, however, invalidate the analogy between the growth of inanimate objects and that of organisms, for the growth of osmotic sacs is from within, and the growth of living plant cells is often simply enlargement by osmosis. Furthermore, there is a greater similarity between the pinching in two of a droplet of mercury and the pinching in two of a bacterium, than there is between the reproduction of a bacterium and that of a man.

There is, however, still another distinction to be made between the reproduction of living beings and of non-living matter. Reproduction may involve nothing more than an increase in the number of individuals, or it may include synthesis. Synthesis, also, is of two kinds, namely, that in which energy is liberated, and that in which energy is used. A full discussion of these interpretations of reproduction is best delayed until the viruses may serve as examples. It is sufficient for the moment to point out that reproduction, regarded simply as a process by means of which two individuals are produced from one, is as true of the non-living world as it is of the living.

Self repair is another vital property which is cited as a distinguishing quality of life; yet crystals "heal" themselves. If the corner of a perfect crystal is chipped off and the broken crystal then placed in a saturated solution of its own kind, the damaged part is repaired, not crudely by merely coating the surface, but in perfect conformity to the original symmetrical pattern.

The classical copper ferrocyanide membrane, formed when copper sulphate comes into contact with potassium ferrocyanide, will repair itself by the formation of a fresh layer of precipitate

over a rupture. It is quite possible that the healing of the torn membrane of a living cell is accomplished in the same way, by the precipitation of a protoplasmic film at the wounded surface.

Among vital processes, respiration, or oxidation, stands pre-eminent as a distinguishing and supposedly ever present quality of living matter.

Oxidation is, of course, always present in metabolically active tissues, but it also takes place in non-living matter. Indeed, the very "vital" respiratory enzymes themselves oxidize sugars when removed from living cells. Respiration, as oxidation, thus has its counterpart in the non-living world.

Far more significant, however, is the fact that respiration need not be present at all in living dormant tissues. There is experimental evidence tending to prove that respiration continues in resting spores, but reliable control material cannot be had for dead tissue absorbs minute amounts of oxygen.

The hypothesis that respiration may be temporarily absent from living matter is particularly applicable to seeds and spores which remain dormant over long periods of time. It seems very unlikely that microscopic cells, such as the spores of bacteria, which remain viable for many years, carry on respiration while dormant, for oxidation at ever so feeble a rate could hardly continue over long periods of time; the amount of organic matter is too small to last. Sclerotia present a similar situation. They are the dormant form of the plasmodia of slime molds. Sclerotia are hard and brittle, and as near absolute dryness as protoplasm can be and yet remain alive, but when given moisture, warmth, and food, they become active. That respiration continues in protoplasm in such a state seems very unlikely.

It has of late been shown that protoplasm may be frozen and yet remain alive. Heretofore this was not thought possible, but it seems that whereas slow freezing results in death, rapid freezing by plunging into liquid air leaves the tissue alive. It is inconceivable that respiration should continue in frozen protoplasm.

To say that respiration may be temporarily absent from living matter is a revolutionary statement. Whether or not it is true cannot be stated with finality. However, the important point

is not whether respiration is or is not present in dormant protoplasm, but that there is no need for it. There is no need for it because respiration is not the distinguishing quality of life. Structural organization characterizes living matter, and as long as it is present there is no necessity for respiration or any other vital process to be active in resting protoplasm.

There may be other paths of escape from a wholly materialistic interpretation of life. Mind, with its associated psychic phenomena, is often resorted to as final and conclusive proof that the living and the non-living are of two worlds. But mind is not a prerequisite of life for plants live.

The second law of thermodynamics is said not to apply to living matter. Non-living systems ultimately attain a state of equilibrium; metabolically active protoplasm never does—but dormant protoplasm may.

Living matter adjusts itself to its environment, it meets contingencies; non-living matter does not—at least only occasionally, as when a falling stone adjusts its position in response to gravity.

Among the most remarkable powers of protoplasm is its capacity to select. Living matter is thus able to violate the law of chance. This is illustrated by the classical experiment which inaugurated stereochemistry. The green mold, Penicillium, cultured on a medium containing dextro- and laevo-tartaric acid selects the laevo-acid and leaves the dextro-form. This power of selection by a mold, between acids so similar chemically that they can be distinguished only by the way they rotate light, is extraordinary, but possibly the mold possesses this power because it has a ferment which reacts only with laevo-tartaric acid.

Similar is the power of selective synthesis by grapes. Grapes form only right hand or dextrorotary tartaric acid; but if tartaric acid is artificially made, an equal number of right and left hand molecules result. Which of the two is formed at any one moment is purely a matter of chance, but in the end there is an equal number of each. Choice, on the part of the fungus in its selection of food, and on the part of the grape in its synthesis of food, may involve no more than the selectivity which is typical of certain forms of non-living matter. Thus, crystals, when enlarging by

accretion, choose their own kind of molecules out of a solution of mixed salts.

The "choice" exhibited by the fungus and the grape is not true choice at all, for the grape can no more turn to making laevorotary tartaric acid than can a machine constructed to make right hand threads turn to making left hand ones. Selective metabolism cannot imply free will. Any implication that freedom of choice is the distinguishing feature of life is therefore not valid. Freedom of choice may exist in higher organisms, but selective metabolism in lower organisms is no proof of it.

A final escape from the tolerance of vitalism which excuses so much, and the severity of mechanism which allows no retreat, is sought by some in the statement that life is the sum total of all those properties which characterize it. Non-living matter may possess this or that vital property but no inanimate system integrates them all. But there is no escape by this route, for if a number of non-living systems together possess all the properties of life, then surely one may possess them all. The possibility of this must be admitted even though its truth is never realized.

As no process distinguishes life, perhaps there is in the chemical composition of protoplasm some ingredient peculiar to it, that is to say, perhaps there is an ultimate living substance, the other ingredients of protoplasm forming an environment, a medium in which the living substance functions.

Protoplasm consists of water, proteins, fats, sugars, salts, and a small quantity of undetermined substances, but none appears to be peculiar to protoplasm alone. However, the proteins are distinctive in many ways. They are capable of a greater diversity and a finer gradation in structure than any other class of compounds. Furthermore, distinctions observed, not only between different kinds of organisms but often between individuals of the same kind, reappear, on chemical investigation, as variations in the respective proteins.

Chemical analyses do not reveal the original substances in protoplasm, or at least not the original state of those substances, for the material which is in the test-tube has only a slight resemblance to that which was once living matter. It is very probable

that the familiar proteins isolated from protoplasm do not exist in that form in the living substance. This would be less true of the stored proteins, but almost certainly true of those proteins which are ingredients of protoplasm. The organization of a protein in one environment is likely to differ from that in another environment.

As the structural configurations which proteins assume are exceedingly great, it is possible that one of these is specific for life.

If the properties of life are expressions of structural peculiarities in proteins, then distinctions between biological species would be a matter of protein specificity. That this is true has been shown by serological work, and by the electric migration of proteins isolated from organisms. Both methods demonstrate that it is possible to establish a plant's phylogenetic position solely by a chemical or a physical analysis of its proteins. Another case in point is that of blood typing in mammals which again suggests that animal relationship is protein relationship.

An encouraging feature of these newer "non-biological" methods of determining plant and animal relationships is that they fully corroborate the older morphological classifications. Morphological characters are thus correlated with, if they do not rest upon, the chemical and physical properties of specific plant and animal proteins.

Added emphasis on the proteins as the distinctive constituents of living matter is had in recent work on viruses.[1] Viruses have been crystallized into a fairly pure state and their molecular composition determined. Because of their large size, chemical nature, and biological properties, the viruses have been given a very significant borderline position precisely midway between the living and the non-living.

In size, the viruses are said to form an unbroken line from molecules to organisms, and thus bridge the gap between what is

[1] I am indebted to a number of associates for information on several subjects here discussed, but to none so much as to Dr. John Northrop, to whom I express my gratitude for his helpful and delightfully stimulating letters dealing with the properties of proteins, enzymes, and viruses; but I relieve him of all responsibility for the deductions made from the information obtained.

alive and what is not. Perhaps they do, in size, but so do other protein molecules known not to be alive. Even non-protein molecules, such as those of cellulose, attain a length which is within microscopic dimensions and therefore equal to that of the smallest bacterium. No great significance can therefore be attached to size alone; it must be accompanied by other properties.

Most of the chemical properties of viruses may be accepted without question provided the virus is pure, and this has yet to be unequivocally established. Viruses can be obtained in a crystalline state; if the crystals are pure, then the virus is a definite chemical entity, that is to say, of one chemical species and consequently homogeneous, of constant composition, and characteristic optical activity. Certain of these properties viruses have been shown to possess, but not all. As it is not yet fully agreed that any virus has been prepared in a wholly pure state, it cannot be unqualifiedly said that viruses are of only one molecular species. Constant solubility is likewise questioned. Lack of purity means lack of homogeneity. More controversial still are the biological properties of viruses.

The viruses are said to be living systems because they reproduce, cause disease, show specificity in regard to host, and induce immunity. That they cause disease is not proof of life, for certain higher fatty acids of tubercle bacilli when injected into animals cause tubercles, though these do not progress as in the natural disease. Similarly, hemolysin gotten from streptococci causes vascular lesions resembling those of scarlet fever, though it does not produce the entire disease.

The capacity to produce immunity is also not proof of life. Typhoid vaccine is simply heat-killed bacteria, and it produces immunity to typhoid fever as well as do the living organisms.

Reproduction is assumed to be the most crucial test of life in the viruses, but viruses reproduce only in the presence of living matter. This may mean either that the virus is an obligate parasite and restricted to a specific host, or that it is synthesized by the living matter which accompanies it. Bacteriophage also increases when in the presence of suitable living tissue, and so do normal proteins and enzymes. An increase in the quantity of a

virus in the presence of living matter is no more evidence of its being a living substance than an increase under like circumstances of admittedly non-living proteins.

The claim that viruses are both pure proteins and living organisms is further weakened by the fact that proteins in solution, at a temperature and acidity favorable for life, hydrolyze slowly into their constituent amino acids. This is the reverse of synthesis or reproduction. The synthesis of proteins requires energy and a catalyst capable of using that energy, and unless both are present, degeneration and not synthesis takes place.

A sharp distinction must be made, first, between reproduction involving simply an increase in number, and reproduction or synthesis involving an increase in volume; and second, between synthesis with loss of energy, and synthesis through the utilization of energy. The name given to each is of little consequence. It is only necessary to bear in mind that these processes are fundamentally distinct.

Protoplasm utilizes energy to make more protoplasm, and the synthesis starts with very simple substances. Non-living proteins, on the other hand, do not use energy in their artificial reproduction, they liberate it, and the synthesis starts with precursors nearly as complex as the proteins themselves: thus, trypsin is produced from trypsinogen and pepsin from pepsinogen by autocatalysis. These reactions are superficially like vital syntheses only in being autocatalytic, but unlike them in that they liberate energy.

Were it known whether viruses and bacteriophages do or do not use energy to synthesize themselves, then and only then could it be said that they have the essential attributes of living matter. If they do not use energy and are formed from protein precursors which are synthesized by the host cells, then they are enzymes. Most investigators consider viruses capable of self synthesis, which includes the assumption that viruses use energy, for proteins cannot synthesize themselves without a supply of energy. The necessity of the second assumption is not always realized. The synthesis of proteins, viruses, and protoplasm from elementary compounds involves the utilization of energy. The forma-

tion of high molecular weight proteins from low molecular weight proteins does not require energy but appears to be an equilibrium, as shown by experiments with hemocyanin.

That the autocatalytic action of viruses is not a synthesis from complex precursors has been maintained on the grounds that there is no cross immunity reaction between the virus, or phage, and the host cell. This objection, however, is not valid because neither is there any cross immunity reaction between pepsin and its precursor, or trypsin and its precursor. In other words, the change from the precursor to the active enzyme also changes the specific immune reaction.

Still another debatable quality which has been attributed to so-called "living" viruses is that of molecular homogeneity. There is no difficulty here if viruses are regarded as non-living proteins of one molecular species, but that living organisms are of one molecular species is very unlikely. Still less likely is it that they are of a single molecule each, as has been suggested. That maximum sized protein molecules are large enough to maintain life may be true, but that molecular homogeneity is characteristic of the living state is unthinkable.

The chemist now recognizes molecular diameters of proteins of over 50 mμ, or 0.00005 of a millimeter, with molecular weights up to 15 million. Such sizes place molecules within the volume range of bacteria. But size alone is a minor property. There may be room enough to permit the multifarious reactions which go on in even the simplest organisms, but more than room is necessary. Whatever life may be, its minimum requirements include the synthesis of food and of protoplasm, and the breaking down of organic matter in respiration. Such reactions, or the potentialities for them, must reside in every living cell. It would appear that a definite minimum number of structural parts are necessary to bring them about. Heterogeneity is a fundamental characteristic of protoplasm whereas homogeneity is typical of molecules. To be sure, homogeneity is a rather vague and a purely relative term when applied to molecules which weigh 17,000,000, as do the viruses. Such molecules possess heterogeneity in so far as they consist of several hundred amino acids

of some twenty different kinds. Indeed, the physicist and chemist of today are confronted with a dilemma, that of deciding whether heavy weight protein particles are molecules or molecular aggregates. Part of the difficulty lies in the fact that there are two definitions of molecular weight in use, the physical and the chemical.

The gram molecular weight, as used by physicists and physical chemists, is defined as the number of grams which, when dissolved in one liter, will give an osmotic pressure of 22.4 atmospheres. The weight of one molecule on this basis is the gram molecular weight divided by Avogadro's number, which is 6×10^{23}. According to this definition any particle in a liquid is a molecule; a marble or a cell is just as much a molecule as the sugar molecule. Direct experimental evidence for the correctness of this view was furnished by studies on Brownian movement and molecular reality, and has been accepted by practically all physical chemists. A virus particle would become a molecule under this definition whether it were living or not.

Organic chemists, on the other hand, define molecular weight in terms of the smallest quantity of a substance which can be taken without changing its chemical properties. On this basis it is more consistent with chemical thinking to regard virus particles, and certain other ones of protein, as aggregates. But the organic chemist's conception of a molecule, though a very useful abstraction, cannot be shown by direct experiment to exist. We are, therefore, forced to grant that certain large "particles" of protein may be molecules.

After all, the controversy may be merely a matter of terms. However, it is true that the rigid laws of the old chemistry are weakening. Mammoth protein molecules are fully recognized today, and they will possess heterogeneity of a kind because they are a mosaic of amino acids; but that this heterogeneity is sufficient for life, the biologist finds it difficult to accept; the chemists and physicist do so more readily.

The biologist is unwilling to accept the heterogeneity of a single, heavy-weight, protein molecule as sufficient to support life, because just as the physicist regards an atom as a collection of

electrons, and the chemist regards the molecule as a collection of atoms, so does the biologist regard protoplasm as an organized aggregate of molecules. No matter how complex an electron may become, it can never be an atom, nor will complexity in an atom ever make a molecule out of it.

Of any organized living system it is possible to ascertain that minimum in complexity which yet permits life to exist. Two thousand cells are the minimum necessary for disorganized sponge tissue to reassemble and form a new sponge; and four-tenths of a millimeter, or one-fifth of the original size, is the threshold for the regeneration of Clavelina. In the same way will there almost certainly be a threshold in molecular number for the minimum living aggregate. To abandon heterogeneity as a fundamental quality of protoplasm would be as difficult for the biologist to do as to discard the concept of the cellular structure of higher organisms. A particle of living matter of one molecule, or of one molecular species, is biologically an unsound concept, and there is as yet no unequivocal experimental evidence to give it substantial support.

A more acceptable and very significant feature of the recent work on viruses is the added emphasis on the proteins as the basic substance in protoplasm.

Proteins are distinguished one from the other by structure. Two protein molecules may consist of the same number and kinds of structural units, yet differ because of a different arrangement or distribution of their parts. Structure, likewise, distinguishes the living protein aggregate from all non-living proteins.

The structural organization which raises living matter above the level of the non-living is not known in all its details, but certain striking and fundamental features of it are known. Work on proteins has shown that the molecule, and its units the amino acids and polypeptide chains, are linear. Linkages between side chains permit the construction of a network which gives to proteins and to protoplasm properties typical of jellies, such as elasticity, anomalous viscous flow, and extraordinary powers of imbibition. The jelly-fish is 96 per cent water, yet when stranded on the beach it can support a hundred pound weight.

After an hour or two in the sun it will be reduced to a thin film by the evaporation of its water. Such qualities owe their existence to a mesh of fibrous units. Structual continuity is characteristic of all jellies, and of protoplasm.

A further important quality of proteins is their great diversity in structural organization. If proteins determine genera, species, and individuals, then there must be enough kinds of them to account for all the diverse forms of living matter on earth. A simple protein of low molecular weight contains some 50 amino acids. The permutations possible in the molecule of such a protein are almost infinite (10^{48}), provided no limiting conditions exist. There are certain restrictions, for the number of possible permutations of a protein must depend upon the ratios and frequencies of each individual amino acid; nevertheless, there still remains a vast number of possibilities in the structural organization of the heavy-weight proteins of which organisms are made.

The biologist finds additional need for continuity in living matter other than the purely physical properties of protoplasm. Genetic continuity can persist only if the linear arrangement of genes remains unchanged. In other words, continuity in inheritance depends upon structural continuity.

The structural organization characteristic of living proteins is specific, but within its kind it possesses great diversity. It is, so to speak, of one genus but of many species, of one class but of many sub-classes. The stereochemical qualities of the proteins permit sufficient variety in plan to give to each species, indeed, to each individual of a species, its own configuration of structural parts, its own protein individuality.

A second structural requirement of living proteins is that of adjustability. The structure cannot be a fixed or rigid one, for protoplasm is often in a state of motion. It sometimes flows with great rapidity. Its structure must, therefore, be facile, its parts capable of repeated readjustments. What, then, can be the meaning of a postulate which tells of a structure that is specific but not fixed? It is impossible to impart motion to the riveted network of a bridge, but a clock is built in such a way that its parts are in motion and yet maintain their positions

relative to each other. Just what this means for colloidal jellies, and for protoplasm, is well illustrated in the behavior of two soap solutions.

Samples of sodium stearate from different manufacturers were found to behave differently; one showed true viscous flow, or Newtonian behavior, whereas the other showed anomalous flow by deviating from the Newtonian law of viscosity. Further investigation revealed that the lawless soap was elastic, even when in very dilute solutions, whereas the other soap showed no elasticity whatever, even in high concentrations. The elastic soap was capable of supporting a metal particle though its viscosity was but twice that of water; the inelastic soap failed to do so at a viscosity 400 times that of water. Microscopic examination revealed long tenuous crystalline fibers in the non-Newtonian and elastic soap, whereas the Newtonian and inelastic soap proved to be built of granules. The interpretation is obvious; an intermeshing network of crystalline fibers gave to the one soap elastic qualities and sufficient rigidity to hold a metal particle in suspension. The granular structure of the other soap was incapable of imparting either of these properties to its solution. A brushheap is elastic; a sandpile is inelastic.

The elastic soap solution, being dilute, flowed quite freely, but perfect fluidity, that is to say, Newtonian behavior, was prevented by structural continuity. Non-Newtonian liquids are socalled because true viscous flow is interfered with. Non-Newtonian behavior is due to a lack of freedom in movement, and this, in turn, to the intermeshing of fibers. Flow brings about a shifting in the positions of parts, but structural continuity is maintained; the elastic qualities of a soap solution remain undiminished by repeated pouring.

That living matter possesses a continuity in structure which only fibrous units can adequately impart to it, has long been the conviction of biologists. The thought was expressed by earlier workers in such terms as "spongioplasm" and "reticulum", and by later ones in "cytoskeleton". The concept is used to explain why protoplasm will, of itself, pass through parchment paper with pores 5×10^{-5} mm in diameter, but if pressed through the much

larger pores of silk, 5×10^{-2} mm in diameter, it will not survive, thus indicating the presence of a fibrous structure which, if destroyed, means death.

The postulate here set forth, that living matter owes its vital qualities to a specific arrangement of parts, is a postulate only in so far as its biological aspects are involved. The physical chemist has long been familiar with the crystalline molecular fibers of which elastic jellies are built. These molecular fibers are protein chains; they may be outstretched, folded, or helically coiled.

That the physical properties of jellies, such as anomalous behavior, elasticity, and extraordinarily high imbibing powers, owe their existence to the intermeshing of fibers, is reasonably evident, but that vital qualities result from such a structure is not immediately obvious.

The proteins of which protoplasm is made acquire the property of life through a specific arrangement of parts into an harmonious whole, somewhat in the same way that a clock attains the unique quality of time-keeping through the perfect coordination of its parts. The wheels of a clock stand in a definite relationship to one another, and unless so arranged the clock will not function. Regularity of motion is the specific quality which has arisen as the result of perfect coordination. This new property is an attribute which is not possessed by any individual part of the clock nor by all parts unless they are assembled in a specific way. As each wheel of the clock is put into place, the mechanism increases in complexity, but the capacity to keep time is not acquired until the last wheel is in position. The placing of the last wheel is a no more intricate act than the placing of any other wheel, yet the moment it is done, a new property suddenly comes into being. The acquisition of the capacity to keep time is a distinct step elevating the clock from a class of functionless mechanisms to a class of organized mechanisms possessing the unique quality of time-keeping. Similarly has non-living matter acquired life through structural organization. The attainment of this function is a discrete step and not a gradual transition; that is to say, the change from the non-living to the living is not continuous. Life has come into being suddenly. Such a postu-

late is no more vitalistic than that which ascribes the origin of the function of time-keeping to the placing of a wheel in a clock.

Modern biology is opposed to the view that life is at a level which is a distinct step above the non-living world because this means that the transition is not a smooth and continuous one. The implication that natural phenomena are discontinuous is also not in agreement with the philosophical physics of the immediate past. Faraday sought to prove the unity of nature. He believed that all natural phenomena are governed by one set of laws. Subsequent research has emphasized this outlook; thus, though the electromagnetic spectrum includes forms of energy apparently as diverse as heat, light, X-rays, and cosmic rays, the diversity consists in nothing more than a difference in wavelength, and the transition from one form of energy to another is continuous. However, a kind of discontinuity exists in that though the same laws apply to all systems there is an emphasis of certain laws in respect to certain systems. This is illustrated in the atomic, colloidal, and celestial worlds.

There is complete continuity in laws between the atomic, colloidal, and celestial worlds, but the continuity is feeble at certain points. There is, therefore, discontinuity of a kind. Thus, the stability of colloidal suspensions is known to be due to electrical charges, and that of celestial bodies to gravitational forces. In other words, whereas both colloidal particles and cellestial bodies are electrically charged, and both are obedient to the law of gravity, collision and settling of colloidal particles are prevented by electric charges on the particles; but the collision of celestial bodies is prevented by centrifugal forces, electric charges playing no appreciable part. And although gravity operates in colloidal systems, it is a very weak force compared to electric repulsion; with celestial bodies the reverse is true. Consequently, though both worlds are subject to the same sets of laws, the stability of the two systems depends upon different forces.

Discontinuity in nature receives further support from the new physics which shows that certain bodies keep within definite ranges in size and mass. The old physics of Newton and Maxwell

was built upon the concept of continuity. The new physics recognizes that electrons and atomic nuclei do not reach the dimensions of atoms, that there are no bodies intermediate in size between the largest known atomic nucleus and the smallest atom. The modern concept of light is another illustration of discontinuity in nature. Light is emitted in discrete units, or quanta, and not in a continuous flow.

In a similar way the transition from the non-living world to the living is not completely continuous. The change is a sudden one. Structurally, the step may be small, yet biologically it is great. In the case of a clock, the last step in construction is slight; it involves only the proper placing of a single unit, but once this is done, a new function comes into being; for the clock, it is the capacity to keep time; for protoplasm, it is life.

The uniqueness of living matter, that which distinguishes it from the non-living, rests on structural features resident in the proteins of protoplasm. These structural features are characterized by a coordination of parts which make of living matter an organized and harmonious whole. It is the uniqueness of this structural organization which raises protoplasm to a higher level in the scheme of things.

Unwillingness to accept a materialistic interpretation of life is often a purely emotional one. We are told that the one-celled animal, Amoeba, meets contingencies which carries us beyond science, whether we like it or not. Amoeba does meet contingencies but this does not carry us beyond science. One biologist objects to the thoughts of his mind being regarded simply as a "rattle of machinery". Perhaps it is the thought that *his* mind is the rattle of machinery which disturbs him. The doctrine that ideals have no effect on behavior is labelled a "monstrous absurdity". But the materialist finds no fault whatever with the statement that ideals affect behavior. Functional organization has been described as a system of ethics more characteristic of a savage state than of a civilized one. This is absurd, quite aside from the fact that a severe mechanist may yet be more humanistic in his attitude than the most sentimental vitalist. After all, these emotional objections have

nothing whatever to do with either experimental facts or philosophic reasoning. To turn the phrase of the vitalist, whether or not we like materialistic interpretations of vital processes, has nothing to do with their reality.

The materialist must, in turn, control his too ready tendency to simplify the living mechanism. It is not at all likely that protoplasm will be synthesized in the near future. It is not true that all intermediate stages between the non-living and the living have at last been found, nor that the transition from the non-living to the living is wholly continuous.

In ascribing the attribute of living to an organized state of matter characterized by structural continuity and a specific arrangement of parts, living matter is elevated to a plane above the non-living world without the introduction of any vitalistic concepts.

University of Pennsylvania
Philadelphia, Pa.

Is Nature Rational?

BY

RALPH B. WINN

MOST words are like small vessels with constantly changing contents. Life does not wait for adjustments in language, but seeks to give an immediate solution to its most imperative needs and interests. It builds up and sometimes destroys. It is a panorama in flux.

Consequently, there comes every once in a while a moment when traditional meanings become so overgrown with the moss of ages that they stand as a handicap for clear thinking. Then it is advisable not to continue studies along customary lines, but rather to make a determined effort toward an analysis of basic concepts in the Cartesian fashion—by purging our minds of all the accumulated presuppositions, by undertaking a re-examination of meanings, by unlearning and re-learning. Among the terms which have reached this stage of obsolescence today is speculative "idealism". Its connotation has become so vague that some scholars, R. B. Haldane among them, urge us to discard it altogether, while others, with J. B. Pratt, beg somebody to explain what idealism really means. Anyway, in S. Radhakrishnan's phrase,[1] "the stage seems to be set for a fresh statement".

The controversy between the idealists and the materialists is so old and persistent that any attempt to end it once and for all should appear as hopeless. Time and again individual thinkers claimed to have dealt a decisive blow one way or another. But

[1] *An Idealist View of Life*, 17.

the problem has apparently a thousand lives, because no sooner do the idealists pronounce materialism dead than their opponents arise in a new form, under another name, in connection with some unforeseen issue. And no sooner do the materialists gain confidence and decry idealism as superstition than their adversaries knock at the back door. Thus, the controversy goes on unabated.

That the two schools or attitudes are, in some sense, opposite is demonstrated by their enduring antagonism. What, then, is their fundamental distinction? It has been asserted that, whereas materialism seeks to account for all phenomena in terms of matter and motion, idealism tries to explain reality in terms of the mind or ideas. According to A. Hoernlé's testimony,[2] "mind, in some sense, is the hero of every idealistic story".

In its most extreme form, idealism flatly denies the independent existence of material bodies. The 18th century philosophers, following Berkeley, strove valiantly to interpret reality as derivative from sensory and reflective experience. Their efforts were, however, judging by the results, unsuccessful if not futile. After Kant, himself a "critical" idealist, refused to commit himself and assumed an agnostic position as regards the things-in-themselves, radical subjectivism was practically abandoned. Nowadays, you will not find, indeed, three noted thinkers who continue faithfully in the footsteps of the good bishop of Cloyne. As C. L. Barrett said in the name of modern philosophers,[3] many genuine idealists believe that the reliance on the Berkeleian principles is mistaken. "The fact is that a large (he should have said: an overwhelming) percentage of idealists do not and never have relied upon the principle of *esse est percipi*, and that to these idealists, if not to all (!), it does not constitute an intrinsic part of idealism".

The fundamental difficulty of subjective idealism—to account for the multiplicity of minds and thus to escape the absurdity of solipsism—has never been satisfactorily overcome. A rather

[2] *Idealism as a Philosophy*, 80.

[3] "Is Idealism Realism?", *Journal of Philosophy*, 1933. The parenthetical remarks are mine.

vigorous attempt, no doubt, was made by Hegel and some of his successors to rescue idealism from its subjectivist faults. He postulated that the minds function under the guidance of the Absolute Idea—or, more exactly, that their operations represent minute steps in its development—and thus reduced men's reasoning to what may be appropriately designated as "spiritual mechanism", in which freedom of will, as ordinarily understood, plays a subordinate and hardly intelligible rôle, much to the detriment of the idealistic attack on materialism. At the same time, Hegel found it necessary to take a step away from Berkeley and Kant back to Plato and Aristotle and to admit that the external objects are no mere appearances, that they do not depend on being perceived. It was in objects, rather than in personal minds, that he found reason in operation. This objective idealism was, furthermore, modernized by being blended with the notion of dynamic purpose, in the form of the dialectics of Spirit; in brief, it taught that both minds and things participate in the realization of a cosmic Ideal. In Hegel's vision, the world became a colossal thought process moving irresistibly towards the fulfillment of the logical order.

This unusual doctrine, powerfully presented in Hegel's detailed elaboration, nevertheless failed to satisfy philosophers trained in respect for science. For "objective idealism", despite its claims to the contrary, was never truly objective. Its subjectivism was merely spread all over the cosmic scene, yet the underlying worldview remained essentially, as it must remain in all idealism, a spiritualist interpretation of nature. Instead of working within many subjects, Spirit, as one Absolute Idea, was assumed to animate everything in reality. But, pluralistic or monistic, idealism persisted in interpreting the world in terms of the mind. In discarding subjectivism—reluctantly, no doubt—our contemporary idealists tacitly confess their inability to comprehend the cosmos as determined *solely* by mental states, human or divine. If, then, neither Berkeleian subjectivism nor Hegelian spiritualism finds much favor with the 20-th century thinkers, what else can idealism mean?

Nevertheless, the old tradition, rich in genuine insights and

also in respectability, will not surrender without further struggle. The newer trend is to pass lightly over the implication of spiritualism and to state emphatically that the world is, above all, intelligible and orderly; that it is built around the high significance of values; and that it is thoroughly teleological. Thus, ideal-ism tends to replace idea-lism. Now, this view is distinctly a retreat from the older position of uncompromising hostility toward materialism, the more so that materialism, in its scientific outlook, itself asserts that things are determined by strict laws discovered in the progress of knowledge; that values play an important rôle in it, particularly in human history; and even that reality manifests meaning and growth. Does this imply that idealism is ready to capitulate and prepare itself for a compromise with some milder form of materialism, say, with realism or naturalism? This seems at least to be the implication of such theories as emergent evolution (S. Alexander, C. L. Morgan, etc.) or cosmic evolution (J. E. Boodin).

Whatever novel form or wording idealism might acquire, one thing cannot be disputed and should be brought clearly to the foreground of discussion. It is, namely, that, beneath all the agelong controversies between the two philosophic schools, there lies a question of paramount significance: *Is nature rational?* This question is entailed, for instance, in the following frank words of Professor C. L. Barrett: "When the idealist says that the universe is Mind, we presume that everyone who possesses any acquaintance with the history of philosophy knows he does not mean that the universe is a conscious thinking being like ourselves. He does mean that it is an intelligible universe, whose systematic structure is no mere mechanism, but expression of the principles of rational order and value which are also the constitutive principles of intelligent organization of our own experience".

In the question "Is nature rational?" the issue does not lie, of course, in the word "nature," by which we all understand the interdependent totality of beings, things, relations, actions, changes. There is hardly much room for disagreement on this point. The whole issue resides in the meaning of the word

"rational"—for, while the materialists confine the term to human experience alone, the idealists apply it to the universe. In fact, the concept of rationality is the alpha and omega of every idealist teaching. Consequently, the following argument will be devoted exclusively to a thorough analysis of rationality.

What has made people believe that the external world is, in some sense, rational? As mental functions have never been observed in inorganic matter, the origin of the assumption is to be looked for in speculation. In this instance, it has been determined, as I am going to show, by the anthropomorphic tendency of human thought, two elements of which are worth pointing out, one an outgrowth of everyday experience, the other a distinctive product of philosophy.

Take an ordinary field flower. Its structure is truly a masterpiece of creative forces of nature. Every detail seems to have been carefully planned for growth from one stage to another. Its color and odor are well designed to attract insects who, quite involuntarily, carry the pollen to fertilize a similar plant. There is considerable waste in the process, of course, but flowers are well prepared for it. As soon as fertilization is accomplished and seeds are ripe, the function of life has been fulfilled for one season. And when one compares this splendid arrangement with the clumsy work of men, one is likely to conclude that, no matter how wise men might become, they shall never surpass nature in wisdom, except by learning from it. No wonder, people are apt to perceive the hand of God in things like field flowers, but not in things like war, corpses, mud, or stench. Yet there is no need to invoke the power of a superior Genius to explain a few selected phenomena appealing to us because of their beauty or complexity. They are no more and no less wonderful than anything else, except in comparison to human creations and in regard to man's interests and tastes. No real phenomenon requires, for its explanation, a supernatural interference, but can be accounted for in terms of natural forces and laws. And indeed, Darwin and other biologists have demonstrated that the structure and function of plants and animals is merely a result of slow selection and adaptation to conditions of earthly

life. Instead of mystical rationality, the scientist finds simply organic *adjustment.*

The second form of anthropomorphism responsible for the idealist interpretation of nature is found in philosophy itself. Humanity is old, but it did not occur even to the wisest of men that it is possible to create a systematic discipline helping them master nature, until the sages of Greece grasped the implications of abstract reflection. At first, they were encouraged in their speculations by the joy of thinking and by the sense of growing power. It was only in the age of Parmenides, Anaxagoras, Socrates, and Democritus that they began to appreciate the superiority of reason over the senses. It was left, however, to Plato to formulate the basic principles of idealism. The work of his predecessors and, particularly, of Socrates, his teacher, convinced him that, contrary to the opinion of the ignorant, logical thought is the way of understanding. In *Crito,* he put this conclusion in Socrates' mouth, saying: "I am and always have been one of those natures who must be guided by reason, whatever the reason may be which upon reflection appears to me to be the best". What, then, is the function of reason? The senses give us merely data passing with the moment. As such, they teach us nothing, because transient phenomena and experiences never return. Only if we catch the imperishable in them, can we learn from them. This imperishable something, which cannot be seen or heard, but can be thought of, constitutes everything that is of value in knowledge and that gives man the power which, acquired bit by bit, enables him to mold the environment. Through the process of abstraction from transient data of observation, man is enabled to comprehend concepts conveying the meaning of Reality in its various interconnected phases. These concepts are, however, no mere artifacts of human imagination. They are derived from, and apply to, nature herself. Insofar as all enduring knowledge of nature is revealed through concepts, they must function in, and dominate in some sense, everything that happens. These elements of permanence, both in the external world and in the mind, were called *Ideas.* Once thoroughly grasped, thought Plato, the Ideas had to be recognized

not only as superior in value to passing images of observation, but also more real. For the transient data of the senses exist only for a moment and then are no longer; but the Ideas, though seemingly arising and perishing in their particular manifestations, nevertheless recur and thus demonstrate their lasting reality. Consequently, things of sensory perception are like shadows of true reality, that of eternal Ideas.

It cannot be denied that Plato's insights into the mind and nature were lofty and magnificent. And yet centuries of philosophic and scientific experience put at the disposal of the modern thinker advantages that permit him an adequate evaluation and criticism of one of the greatest systems of thought. Without detracting anything from the historical contributions of Plato, we can say that his beliefs are no longer acceptable in their original form. We acknowledge willingly, of course, that the laws of nature can be grasped only by the mind assisted by the senses. We concede also that these laws, as understood by men, are invisible and inaudible and that they can be discovered only through generalization and abstraction from phenomena. We have learned, however, that thoughts have a much closer connection with the body than Plato could have ever suspected. We no longer contend that they proceed from that entity inhabiting the organism, which was traditionally called "the soul". We are inclined, in fact, to doubt its very existence. Instead, we have discovered the constant dependence of thoughts and emotions on the brain, the function of which is to adjust one's behavior to whatever conditions are presented by one's environment. Even the most profound reasoning is recognized today as a way of solving problems, as adaptation. The brain itself is, in its biological and psychological aspects, nothing other than an organ of *adjustment.*

Thus, we are confronted with the alternative questions: Is nature rational? Is reason natural? At a first glance, the alternative is artificial or false, because, as one might maintain, both questions admit of affirmative answers. But this is not what I mean. What I do mean is that, without denying the more or less permanent nature of scientific laws, it is possible

to regard them as possessing the characteristics of reason even apart from the human knowledge of them—which is an idealist's view. Or else it is possible to interpret human reason as a biological tool shaped by nature in the course of ages and, in its turn, discovering in speculation those very laws which had created it—which is a materialist's contention.

The idealist insists on reading the mind into nature, as follows: "Could nature produce something fundamentally alien to herself? Could she put into her products what she herself in no measure or degree possessed? If nature could produce minds, must it not be because she possessed those characteristics with which she endowed them? Or, to speak less metaphorically, if human minds are actually parts of nature, must we not suppose that there is a likeness between the rational order which governs their thinking activities and the order which regulates nature in its other spheres or aspects".[4]

The strange thing about this typical argument is that no thinkers but the idealists have ever contended that the mind is alien to nature. The materialist is the first person to admit that reason is natural and that the mind has been slowly shaped in the processes of evolution. Novel forms of existence arise from time to time, not because they have pre-existed in reality in the form of "rationality", "will", "personality", "spirit", "soul", and what not, but because all combinations of matter and energy have not, and never will be, exhausted. The materialist argument, in this general form, is not new and has been clearly stated as early as the very beginnings of modern philosophy, namely, by Galileo Galilei who said: "Nature did not make human brains first, and then construct things according to their capacity of understanding, but she first made things in her own fashion and then so constructed the human understanding that it, at the price of great exertion, might ferret out a few of her secrets".[5] It is a well-known tendency of idealism to side with transcendental metaphysics, while materialism tends to affiliate itself with scientific empiricism.

[4] C. L. Barrett, *Philosophy*, 306.
[5] *Opere*, VII, 341.

There is no reason for me to conceal that I accept wholeheartedly the latter position as based preeminently on verified findings of observation and thought. I realize, of course, that materialism is far from perfect at the present stage of its development, and even the term itself is not free from ambiguities. Nevertheless, the foundations of materialism are rooted in facts and practice; and the trend of scientific development since the 17th century indicates that science itself, despite its occasional lapses into mysticism, is about to confirm the principal assumptions of materialism. Idealism, on the other hand, has its historical source in anthropomorphism and mysticism, and, as the latter is being slowly eliminated from the domain of sound thought and observation, idealism finds itself more and more on the defensive.

It seems to me that idealism is quite wrong in ascribing to reality the qualities of thought, as if reason created nature and not the other way around. In doing so, idealism projects human traits into the external world. An analogy, paradoxically derived from Plato himself, will illustrate the point. Suppose we paraphrase and slightly modify Plato's famous story of the cave,[6] to read this way:

> Behold! human beings living in a hall with mirrors of colored glass instead of walls. Here they have been from their childhood, and have their legs and necks chained so that they cannot move, and can only see before them, being prevented by the chains from turning round their heads. Above and below them bright lights are shining, and between the lights and the prisoners there is a raised way; and you will see, if you look, a low wall built along this way, like the screen which marionette players have in front of them, over which they show the puppets.
>
> I see.
>
> And do you see, I said, men passing along the wall carrying all sorts of vessels and various materials, which appear over the wall? Some of them are talking, others silent.
>
> You have shown me a strange image, and they are strange prisoners.
>
> Like ourselves, I replied; and they see only the reflections of things

[6] *The Republic*, Book VII.

and people, including themselves, which are things in the mirror before them.

True, he said; how could they see anything but the reflections if they were never allowed to move their heads?

And of the objects which are being carried in like manner they could only see reflections?

Yes, he said.

And if they were able to converse with one another, would they not suppose that they were naming what was actually before them?

Very true.

And now look again, and see what will naturally follow if the prisoners are released and disabused of their error. At first, when any of them is liberated and compelled suddenly to stand up and turn his neck round and walk and touch real objects, he will suffer great bewilderment and uncertainty; the solidity of things will distress him and inflict actual pain in a rough contact. He will be unable to recognize the realities of which in his former state he had colored reflections; and then conceive some one saying to him, that what he saw before was an illusion, but that now, when he is approaching nearer to facts and his eye is turned towards more real existence, he has a clearer vision—what will be his reply? And you may further imagine that his instructor is pointing to the objects as they pass and requiring him to describe their qualities—will he not be perplexed? Will he not fancy that the reflections which he formerly saw are truer than the objects which he now touches?

Far truer.

We need proceed no further. It is a familiar truth that at all times man's thought tends to run along anthropomorphic, or even egomorphic, lines. In the past, he endowed natural forces with human-like characteristics (animism); populated the world with gods thinking, feeling and acting in a manner reflecting man's own nature or tribe's own culture; placed himself into the center of the universe; and identified the destiny of the cosmos with that of humanity. Nature thus became a mirror reflecting human qualities, desires and aspirations, while men persistently failed to recognize anthropomorphic distortions of reality in their views. Similarly with reason. Reason is man's tool of knowledge, a way of solving more or less complex and remote problems of life. It enabled him to comprehend even the laws of nature.

But is it necessary to make the external world partake, in one's own mind, of the human way of experience and then to "discover" that, remarkably enough, nature is rational? Reality (as existence) and truth (as an adequate knowledge of it) have something in common, no doubt—for how else could knowledge be possible?—yet it is sheer anthropomorphism to call nature rational, rather than reason natural, even when this is done by noted physicists, such as J. Jeans visualizing "the Great Architect" and "Mathematician"[7] or A. Einstein saying: "It is enough for me . . . to try humbly to comprehend even an infinitesimal part of the intelligence manifested in nature".[8]

It is about time to recall once more Francis Bacon's famous warning: "The Idols of the Tribe have their foundation in human nature itself, and in the tribe or race of men. For it is a false assertion that the sense of man is the measure of things. On the contrary, all perceptions as well of the sense as of the mind are according to the measure of the individual and not according to the measure of the universe. And *the human understanding is like a false mirror*, which, receiving rays irregularly, distorts and discolors the nature of things *by mingling its own nature with it*".[9]

In order to comprehend the true nature of things, let us ask a few specific questions. When is thought rational? Obviously, when its conclusions are adequate to the problem in question. And the problems engaging one's thought are all, directly or indirectly, of practical significance, having been stimulated by experience and being applicable to it after a correct solution. The human mind is a creation of earthly environment. For countless ages it was being shaped by experience, natural selection, and numerous other forces, until finally it acquired the present capability to perceive, to imagine, to reason. Man's power of understanding may as yet be very limited and fickle, but it was again nature that had set these limitations. She gave her human children but a few sensory channels along which

[7] *The Mysterious Universe*, 12.
[8] *Living Philosophies*, a Symposium, 7.
[9] *Novum Organum*, aphorism 41. Italics are mine. R. W.

cognition was permitted to them; distorted their knowledge by providing sensory experience with a standpoint shifting in time and space; subordinated thought to emotion; and organized their logic in agreement with the dictates of the natural order. Truth itself, as a distant limit of intellectual wisdom, became nothing other than nature disguised in the symbolic forms of human knowledge. In his ignorance and vanity, however, man customarily regarded reason as his own achievement and virtue, and applauded the world whenever he succeeded in noticing its kinship with the operations of the mind; or, in an outburst of misguided insight, he projected his own qualities outward and discovered, in the ecstasy of anthropomorphic mysticism, Reality permeated with Thought.

Yet the fact remains that reason is neither a prerogative of wise individuals nor a free gift of Heaven: a rational thought is simply that thought which is in sufficient agreement with the laws of nature, that thought which is well adjusted to it. Adjustment is often factual, but rationality is a human name for the recognition of it.

When do we regard a man's behavior as rational? It is not intelligent to address workers as if they were professors, or adults as if they were children; nor is it a sign of wisdom to take an umbrella on a clear day and to leave it at home when it rains; and it is definitely stupid to put salt into one's tea and expect it to be sweet. Rationality manifests itself, not in disregard of particular situations, but rather in insight into their practical meaning. The main requirement of intelligence is to modify and adjust one's behavior according to time, place and circumstance. Unless this is done, one's behavior results in inconsistency, blunder, failure.

When do we regard an institution as functioning rationally? Clearly, when it satisfies human needs and expectations. And as requirements alter with time and country, every institution should be adapted to social change. The institution ceases to be rational in proportion to its social lag or obsolescence. Its efficiency or inefficiency is invariably relative to the general situation. That is why feudal practices normal to, and con

gruous with, the older ways of living could not "work" under the later, capitalist, structure of society; and the institutions of the laissez-faire period cease, in their turn, to "work" under contemporary conditions, insofar as they are not quite adapted to meet new social phenomena and popular demands.

When do we find rationality in an organism, say, in a field flower? If climatic conditions prevent the normal development of the plant, it will not reach the stage of maturity necessary for fertilization; and if the species, because of some change in environment, fails to attract insects and, consequently, to produce seeds, it is bound to perish. What we call "rational", in these and similar cases, is nothing other than sufficient adjustment of parts to the whole, of the preceding stage of growth to the subsequent ones, of the individual organism to its environment.

Is the external world, as exemplified by activities of the chemical elements and by the motions of heavenly bodies, rational? The history of science demonstrates that nature is a coherent whole. By this I do not mean that there is no conflict in the universe. Quite the contrary, motion, change, and the conflict of forces are its most conspicuous characteristics. Nevertheless, we know full well—and our knowledge is supported by the entire evidence of science—that the laws of nature are not self-contradictory. If a theory does not account for every phenomenon in its field and admits of exceptions, we declare it inadequate or faulty, obviously in the belief that the laws of nature work always in a definite and exact way. Shall we say, therefore, that nature is perfectly harmonious? No; for surely absolute harmony would imply changelessness. Yet change is everywhere. What can it mean? Clearly that nature is constructed (this statement of mine has no idealistic implications) in a manner in which the balance of forces is continually lost and re-established. There are both adjustment and lack of adjustment in nature. There are both rationality and irrationality in it, if we wish to make a concession to human language. And laws of nature stand evidently for rules of adjustment, without which there would be a universal chaos beyond human understanding.

Finally, is man a rational animal? According to a traditional assumption, he is. But the statement does not contain the whole truth, for there is a basic contradiction in it. Professor H. L. Hawkins, a distinguished paleontologist, was right in saying that man is the only "irrational creature", as any worm or fish, thanks to its instincts, is better adapted to its habitat. Man's utter helplessness is vastly compensated, however, by his enormous *power of adjustment*, of which brain is the organ, which helps him not only to gain the upper hand in the animal world, but even to combat and master natural forces themselves. Where a lower animal perishes as a victim of its own splendid adjustment to the given but passing environment, man adjusts and re-adjusts himself to change. He has learned to live in arctic lands as well as in the tropics, to travel under the surface of the sea and above the ground and even above the clouds, to measure microscopic quantities and to lift mountains of stone.

There is no need for multiplying our instances any further to see that *rationality invariably implies adjustment* and should be defined in its terms, lest our knowledge be obscured by mysticism and anthropomorphism. That is to say, we should define rationality as a form of adjustment, not vice versa. In referring to an instance of rationality in nature, the idealist actually deals with a case of adjustment. But he is not satisfied with this scientific name. Rather, he prefers to glorify it. He chooses to call a natural thing by a name that would put himself and his kind on a pedestal. That is how he finds reason in the cosmos. When, however, the idea strikes him, in a moment of doubt and weakness, that he is just a speck of dust in the immense spaces of the universe, he drives the bad thought away. It is a pity, after all, to give up his glorious illusion; for it is pleasant to cherish the notion that, in some sense at least, he is still the center of all things—if not because the world has been created specially for his sake, if not because he is God's most beloved son, if not because the sun, planets and stars revolve round the earth, then because the world is guided by those very principles of which he is the most skillful master, by the principles of reason.

Many a noble man suffered because of these illusions. Many

a great struggle of science was waged against these "glorious" illusions. But they are not yet all dead: the bitter fight must be continued to the end. We must learn to call things by their right name. And rationality must be recognized for what it truly is—a form of adjustment, as recognized by men. But then rationality ceases to offer a valid argument in favor of the idealistic interpretation of reality; on the contrary, it becomes a trump in the hands of the materialist.

The above analysis of rationality throws a very definite light on the nature of human mind and also on the basic fallacy of idealism. Deep in human nature there lies the urge for understanding. It is not a desire to know for the sake of knowing, but either an expression of the need to live with a clear purpose or else an expression of the belief that knowledge can be utilized for practical aims of man and humanity. It is the urge for understanding as a means of solving the problems of life, emotional and factual.

However, in his primitive existence, man had all the vigor of emotional yearning, yet little insight into the paramount significance of reason. Because knowledge is essentially human, it contained a great deal of anthropomorphism. And because knowledge is essentially practical, it took, in the absence of adequate and exact information concerning the elemental forces of reality, the form of mysticism.

So mysticism became the "knowledge" of our ancestors. They populated the universe with strange beings of fancy, created in the image of man. There arose, in their beliefs, good and evil deities who had to be propitiated, flattered, appealed to, respected, obeyed.

This primitive attitude persists, to some extent, in modern idealism. Anthropomorphism has taken, of course, an advanced form. The forces of environment are no longer conceived of as spiritual entities, except by naïve, emotional and mystic souls. Mysticism has receded only to vest itself into the scholar's language. It portrays the universe not in the image of man's body, but in the image of man's mind. It declares the existence of Intelligence controlling the world.

Today, when we can account for most phenomena in terms of

facts and figures, even if not to the complete satisfaction of scholars, when we become increasingly masters of our surroundings, it is superfluous and often harmful to persist in our primitive illusions, whether expressed with simple faith or dressed in the language of erudite philosophers.

True as all this is, it may be advisable, to avoid a possible misunderstanding, to say that I do not condemn the idealism *of the past.* I realize full well that it has been instrumental to the development of civilization. Nevertheless today, when science, philosophy and art have largely emancipated themselves from their original mysticism, we ought to face facts more confidently. Instead of dissipating people's energies in the vain search for intangible and transcendent goals, we should comprehend that all our tasks are earthly and human. As far as philosophy is concerned, it is not enough for it to rise to the higher planes of abstraction: it should descend, enriched and emboldened, to the source of its original inspiration—man's pursuit of security and happiness. For a correct interpretation of its mission, we should assimilate thoroughly the idea that "philosophy at all times, like science, has had its roots in man's practical needs".[10] Consquently, whether we are noted thinkers or plain people, it behooves us to concentrate our efforts on solving our immediate problems and on building a better and fuller life among the nations of the world.

College of the City of New York.

[10] J. H. Muirhead, "Past and Present in Contemporary Philosophy", *Contemporary British Philosophy*, I.

Physics, A Vicious Abstraction

BY

HORACE S. FRIES

THE argument of this paper is intended merely as a reassertion of John Dewey's naturalistic thesis that value and existence cannot be separated; and that the traditional concept of science which separates them (and which originated before the advent of science) is vicious. But the writer is not at all sure that he has not perverted the ideas of Dewey beyond recognition. If so, he hopes he has perverted them away from truth, but he fears otherwise. For there is not much hope of "reforming" science; at least, not of cutting and stretching it to fit the Procrustian bed herein prescribed. But if science really does not need reform, the writer is more than willing to submit to the sword of Theseus along with all other marauders and disturbers of the peace.

Perhaps the writer owes his readers—if any—an apology, also. For according to his friendly critics who surveyed this essay in manuscript, the fourth footnote "condemns" him as a child of science. So be it! He hopes he is a friend of science; and the marks which should enclose every condemnation in this day indicate sufficient vagueness and confusion to warrant hope of absolution in case a sin has been committed, one way or the other, against the holy.

I

For the purposes of this paper the science of physics is chosen as a scape-goat, because of the high degree of success it has at-

tained as an exact science. Any science would serve the same purpose but only in proportion to its exactness.

The assumptions are made throughout the paper that meaning is operational and that truth is relative. Probably the second is implied by the first, but this need not concern us here.

By the operational nature of meaning is meant (1) the denial of the idea that ideas or concepts refer to an antecedent existence[1] and (2) that an idea intends certain *prospective* connections or consequences by means of which operations performed will lead from one reality to another or, in other words, by means of which operations will transform one reality into another.

The relativity of truth means that ideas are true in proportion to the success with which they realize their operational intentions and that their truth consists in this success.

Upon these assumptions it is proposed to show that physics is vicious because it is treated as an isolated abstraction. The writer believes that it would be beneficent if generally it were treated differently, but this claim will be considered only incidently.

The reasons why physics is vicious when treated as an isolated abstraction are (1) that to see it as indubitably successful depends on so treating it[2] (usually without admitting such treatment) and (2) that when treated as a concrete aspect of an interrelated and incomplete ongoing process its success is seen to be contingent upon the success of the other sciences including the social sciences.

The successful operations which can be performed by means of the propositions of physics are performed only under conditions of "isolation"; i.e., the laboratory or certain very restricted areas of practical application. But neither in the laboratory nor in industrial applications can it be isolated from the complex web of

[1] To deny that ideas refer to an antecedent existence is not to deny antecedent existence as many critics of instrumentalism believe. The idea of antecedent existence itself refers to certain consequences.

[2] This I take to be a species of the fallacy of misplaced concreteness. But the fallacy is not avoided or corrected by placing the abstraction in a network of other abstractions, however comprehensive the total system may be. It can be avoided only by continuous testing and reconstruction of the abstraction in terms of its actual fruits, its human consequences, what it does.

social-economic conditions which obtain (at least it is not so isolated, which is enough for this demonstration). For the very same propositions which yield success in the laboratory and a very dubious "success" in industry have consequences outside these realms. But according to the operational theory of meaning, the meaning of a proposition is its consequences. For certain purposes—e.g., for the purpose of experimentation—it is convenient to shut one's eyes to some of these consequences, and to concentrate on the remaining consequences which constitute the *explicit* intention or *intellectual* meaning. But to forget that one has shut his eyes is to entertain the possibility of error unawares.

Suppose that we open our eyes. It follows that we admit the possibility of the presence of a systematic error in the propositions of physics which does not show up in its operations in the laboratory because we "isolate" physics sufficiently and in the appropriate places to keep them out of what we implicitly agree to label "its operations." But if the propositions do have consequences beyond this "isolated" realm, then only a faith in Santa Claus will allow us to believe that these consequences may not be reflected in the apparently isolated realm. Faith in Santa Claus here is the belief (1) that there is an absolute truth, (2) that physics in so far as it is successful in "isolation" reveals this truth in part, and (3) that this truth is of such a nature that it will successfully guide the efforts of all those who piously believe they are in pursuit of it—"though the heavens fall."[3]

But if we reject the notion of absolute truth and accept the operational theory of meaning, we see at once that the consequences of physics beyond the "isolated" realm must themselves be guided as successful operations by human beings, if physics is on the right path.

[3] The dominant ideology of science is supernaturalistic in an important sense. If engineers found it convenient as bridge designers to distinguish this function from actual construction, nothing logically could prevent them. And if they were haunted by a ghost-idea that there is an eternal realm of bridge designs underlying and regulating the world where actual bridges are constructed and eventually decay, then they could, as designers, distinguish between Pure Engineering and applied engineering and refuse to take responsibility for the consequences of their "pure" science outside its "subject-matter." The only trouble with the distinction would be that they could not get by with it.

But the only way humans can guide these "external" consequences successfully, is by learning how they operate. This is the task of the social sciences.[4]

II

Let us examine, therefore, the implications for social science of a relative theory of truth and an operational theory of meaning taken together.

There is no absolute truth. Consequently there is no factor in the human mind which somehow reflects this absolute truth whether it be conceived rationalistically as *a priori* synthetic forms or intuitions or empirically as sensations with a one-to-one correlation between knower and known.

What then will be the test of truth in the social sciences?

In the first place let us note that if we are consistent operationalists, we can admit no place for separate scientific subject-matters. In so far as they are distinguished, it is for methodological convenience. For in our social order, in our world of human

[4] In a certain tautological (but important) sense the successful "isolated" propositions are true regardless of the bearing of the non-isolated realm on them. For the successful operations of an idea which define its truth are *its* intended operations. Consequently, if the "isolated" idea intends certain limited operations and is *always* successful in realizing them, it is "true" regardless of other of its consequences. But, unfortunately however many successful ideas by limitation of their intentions have been made possible by the growth of science (and their number is the glory of science), there is nothing in science which can guarantee that these intentions, limited in just this way, will continue to be relevant. Given our climate of intellectual opinion, they are relevant. But any climate of intellectual opinion is itself conditioned by the wider cosmic weather of social habits or institutions over which man has as yet won no control. It is these which will ultimately determine whether the world is mature enough to manage the human power which has been released by science.

Our climate of opinion is under deliberate attack in many parts of the world. Even in America there are foreboding signs. To choose an example at random, consider a recent best seller, Henry C. Link's *Return to Religion*. The book is an unblushing attack on science and a defense of superstition by one who calls himself a psychologist. Amusingly enough, about the only activities of psychology which are "really scientific", according to Link, are personality and character tests and measurements. Unfortunately there are enough abuses of science and its applications to lend a color of truth to the entire book for its numerous uncritical readers. But the most potent enemy of science is dire want amidst the plenty science has made possible. It is time for the friends of science to become critical of it and all its concrete implications before the attacks by its enemies have made constructive criticism futile.

experience today, consequences interpenetrate and form a complex web of relations. Scientific subject-matters are distinct, therefore, only as abstractions; that is, only as selected aspects of a complex and indeterminate whole. A "separate" subject-matter can be obtained only by shutting one's eyes—knowingly or not—to actualities omitted in the act of abstracting. Hence, abstractions of subject-matters which are necessary in order to yield "success" within a certain subject-matter must themselves be successfully tested before the "success" within the subject may be considered valid. But the only way to test the validity of an abstraction is to see how it works in the totality from which it is abstracted. The test of a successful distinction of subject-matters will be, therefore, the success with which the distinction works in all "fields;" that is, the concrete realm of experience or the social order. The number of publications in the "best" journal within the special field is a test which may be as misleading as irrelevant—except perhaps for promotions.

The basis of a specialized subject-matter, then, is functional. The logical justification of specialization is that because of it operations can be made more successfully throughout society.

The test of truth generally, then, as well as in the social sciences ought now to be evident if the traditional prescientific concept of science is not blinding us. It is how well the operations enable us to perform our functions; i.e., the test is ethical, social. In order to agree as to what is true we must agree as to what is desirable.[5]

As we deliberately set out to discover what is desirable in the light of what is known and what more can be known in the light of what is desirable, we should expect an interaction between the two which will throw light on each. For the argument is that the desirable and the known are not separable. Obviously for a relativism, the desirable, as well as the true, changes with changing conditions. But this fact can vitiate the distinction between the desirable and the undesirable only on the presupposition that the real cannot change. If scientific truths are relative

[5] It ought not have to be argued today that the development of physics and chemistry was operationally desirable (however uncritically this "desirability" may have been conceived) in a struggling, young industrial capitalism.

yet objective, why must a relativity of value force us to subjectivism? The answer is the pervasiveness of an uncritical tradition initiated before the time of Bacon and Descartes but dignified by them as a method and a philosophy of science; the tradition, namely which separated other values from scientific truths. This distinction and separation may have served an important purpose, but the time has arrived when the separation will be the undoing of science unless it is critically reconstructed.

The test of successful scientific functioning is the same, then, as of any special function: how well do the operations succeed. "Success" operationally conceived is no more amenable to arbitrary definition than is the definition of water as H_2O. But a working definition of success can be obtained, if we take a hint from science, only by deliberately attempting to get it; not by divine revelation, not by fiat, and not by blindly attempting to conform to what once were prevalent mores but which have been thrown into confusion by the advent of science itself. The working definition of success can be nothing short of the continuous attainment of the good life and the good society.

The point is that the truths of physics (and, *a fortiori*, of the other sciences) must be acknowledged by a thoroughgoing relativism to be based upon a very shaky foundation until we can see progress as clearly in the development of our concrete ideas of the good life as we have seen it in the development of physics as an "isolated" subject. It is just conceivable that experimental science is on the wrong track.

Nevertheless, those of us who see in science the highest hope of the human race yet discovered accept "workability" or "prediction and control" as tests of truth. But "workability" achieved by arbitrary limitation of the realm in which "true" propositions work is obviously not a very secure test. The naïve attitude of many scientists towards value, questions of good, right, wrong, desirable, social relations, etc.; the attitude which incorporates a mixture of primitive supernaturalism and superstition about human relations in contrast with the attitude they assume in the laboratory is nothing short of pathetic.[6]

[6] Any attempt to combine the orthodox approach to ethics with scientific method will confound confusion. There are two basic ideas brought out in Dewey's *Human Nature*

The proposition that physics does not contain a systematic error which will eventually be its undoing means operationally that if physicists (and social scientists who seem determined to emulate the physicist) would *as scientists* concern themselves with the social consequences of their activities, then control of them would ensue.[7] That is, a tentative agreement on the more desirable in the way of social consequences would be reached; this would react as a test; and new tested operations would in turn react to give a more definite, broader, and better working concept of the desirable.[8]

The argument of this paper can be summarized somewhat symbolically as follows, and is so summarized for the sake of those naïve souls who believe that truth is lodged in words instead of things.

$$p \equiv q$$

where p = any scientific proposition and

q = its operational consequences.

But $q \equiv (r\ s)$

where r = the set of "isolated" operations in the laboratory and

s = the other social, human, ethical consequences.

$$\therefore p \supset q \supset (r\ s)$$

---

and Conduct which point the way to the unification of existence and value. One pertains directly to science. It is the distinction between an awareness of *consequences* and an awareness of an *outcome*. The function of science is not to predict *the* future. It is to reveal different possible futures and ways of attaining them. On the other hand the function of ends and ideals is not primarily to indicate that a future state of affairs should be realized. Ideals and ends are guides to present action. They themselves must be tested and improved in view of how they operate in present action. As Whitehead has said someplace, "The present is holy ground." Woe is Israel, indeed, for there is little hope of science becoming operationally ethical, and there is no hope for ethics without science.

[7] I am not sure but that the antecedent of this implicative proposition is also asserted in asserting the truth of physics; that is, that scientists *will* take such a concern; at least the only alternative condition would seem to be lady luck.

[8] For a tentative suggestion as to an end upon which scientists might agree as a starting point which has a democratic preference see my paper "Method in Social Philosophy", *Journal of Social Philosophy* 3 (July, 1938), 325–41.

But $s' \supset (r\ s)' \supset q' \supset p'$

However, $(r\ s) \supset q \supset p$

And r (in "isolation" *appears* to be true)

$\therefore$ if s (not in isolation can be brought under control comparable to r) then

$$\overset{s}{}$$

And $(r\ s) \supset q \supset p$

But $(r\ s)' \equiv (r' \vee s') \supset q' \supset p'$

That is, since r and s are not separable concretely, the truth of r is *merely* apparent until s is brought under control. If s cannot be made true, then as far as we know r is as likely false as true.

The concluding paragraph of Mr. Julian Huxley's book, *Scientific Research and Social Need*, affords a fitting conclusion to this paper.

> The chief moral of this book, it seems to me, is that science is not the disembodied sort of activity that some people would make out, engaged on the abstract task of pursuing universal truth, but a social function intimately linked up with human history and human destiny. And the sooner scientists as a body realize this and organize their activities on that basis, the better both for science and for society.

University of Wisconsin, Madison, Wis.

Our Knowledge of Other Minds

BY

C. D. HARDIE

IT IS with some trepidation that I am raising again this old problem, for it may well be said that nothing new could possibly be written on it. On the other hand it can scarcely be maintained that what has been written has left the problem in a satisfactory state. Moreover, it seems to me that some work which has been done recently on the nature of theoretical physics can be used to dispose of most of the difficulties, and it is partly in the hope of drawing attention to that work and its bearings on psychology that I am writing this paper.

The problem to be discussed is not the genetic question "how does my mind come to know other minds", but the logical question "what ground have I for believing in the existence of other minds". Of the traditional answers to this question I think the following two are the most important:

I. My knowledge of other minds is based on analogy and induction. I have experience of my own mind, the behaviour of my own body and the behaviour of other bodies. From the analogy between the behaviour of my body and the behaviour of other bodies and from the knowledge that my mind controls (in some sense) my body I proceed to the inductive conclusion that other bodies are controlled by other minds analogous to my mind. Consider now the following propositions:

(a) Any conclusion arrived at by analogy and induction is at best only probable.

(b) My knowledge of other minds is arrived at by induction and analogy.

(c) I am certain that other minds exist.

Considering these propositions individually, I wish to assert all of them, but any two together entail the contradictory of the third. Because of this weakness the second answer to the problem becomes more plausible than it might otherwise be.

II. This is the answer of the Behaviourist.

There are various forms of Behaviourism, but the only one which makes any attempt to deal with the difficulties of the subject is that which has come to be known as Syntactical Behaviourism. This asserts that if a word is introduced into any language some criterion (in terms of a set of operations) must be given for its use in that language. Thus if a physicist wishes to introduce the word "mass" as a property of objects, he must give a set of operations which will enable anyone (at least anyone with a certain amount of knowledge and skill) to decide if a given object has mass or not. Similarly, if a psychologist says that A has a mind where A is the name of an object having the physical properties of a man, he must be able to give criteria which will enable anyone to decide if A has or has not a mind. It is clear that such criteria must ultimately be certain features of the way A behaves, including in this, of course, what A says.

A slightly less extreme form of the same answer is the "Linguistic Solipsism" which has come to be associated, rightly or wrongly, with Wittgenstein. This maintains that I must analyse sentences about other minds into sentences about the behaviour of other bodies, but that I should not analyse sentences about my own mind into sentences about my bodily behaviour.

Difficulties, however, may be urged against both forms of the doctrine which seem to make it no more satisfactory than I. Against the milder form it can be urged that it does not really give us what we want. For I know that other bodies have minds in the same sense in which my own body has a mind, and I ought therefore to analyse sentences about other minds in the same way as I analyse sentences about my own mind. And against the more extreme form it can be urged that I do not

necessarily mean by a sentence involving "my mind" anything at all concerning my bodily behaviour. It therefore seems to me that there is room for an account of our knowledge of other minds which (1) makes our knowledge of them depend in some way on analogy as in I, but which (2) is such that we can be certain that other minds exist as in II. It is such an account that I will try to give.

I shall consider first of all the logical nature of a theory in physics and for a clear exposition of this I am greatly indebted to N. R. Campbell, *Physics, The Elements* (chap. VI). According to his analysis any theory consists of two parts which may be called the hypothesis and the dictionary (the use of "hypothesis" here does not mean anything uncertain). The hypothesis consists generally of statements about mathematical variables and the dictionary consists of statements connecting these variables to observable phenomena. The connection may be direct as when a variable stands for an observable characteristic, for example mass, or indirect as when a relation between variables is asserted to be equivalent to the occurrence of some observable phenomenon. For example, in the Dynamical Theory of Gases the hypothesis consists of statements about variables m, t, (x_r, y_r, z_r) ($r = 1, 2, \ldots n$), etc., and the dictionary relates some of these variables to observable phenomena by asserting, for example, that when

$$m \sum_{r=1}^{n} \left\{ \left(\frac{dx_r}{dt}\right)^2 + \left(\frac{dy_r}{dt}\right)^2 + \left(\frac{dz_r}{dt}\right)^2 \right\}$$

has a given value the absolute temperature of the gas is determined. Theories will be said to be of the first or second kind according as the dictionary relates all or only some of the variables to observable characteristics. The Dynamical Theory of Gases is hence a theory of the second kind and for such a theory to be valuable it must have two properties. (1) It must be possible to deduce new relations from the hypothesis which when translated by the dictionary yield laws which govern observable phenomena. (2) The hypothesis must be formally analogous to the hypothesis of a theory of the first kind. Thus the Dynami-

cal Theory of Gases is valuable because it satisfies both conditions, the theory of the first kind to which it is analogous being the theory of perfectly elastic particles or small billiard balls.

Now in the latter (x_r, y_r, z_r) are measurements in space, t is the time, m the mass and so on; hence in the gas theory we can talk of "molecules" or "perfectly elastic billiard balls" of mass m, and we can say that the observed phenomena of pressure, volume, temperature are explained by the mass and velocities of the molecules. To the question are we certain that gases really have molecules we must answer 'yes' because this has been given a meaning in a way which makes us certain. If it is said that the observed phenomena are not due to very small balls in the same sense of 'ball' in which a pin head is a large ball, I agree, if by 'same sense' is meant that we verify the propositions in the same way. But the phenomena are due to small balls in the sense in which a pin head is a large ball if by 'same sense' we mean that the equations giving the behaviour of the balls are the same.

Put briefly the position seems to be this. For the billiard ball theory we have some mathematical equations (the hypothesis), some observed object laws (laws about the observable behaviour of billiard balls) and a dictionary connecting them. For the gas theory we have some mathematical equations and it is found that these are similar to the equations of the billiard ball theory. We hence introduce certain terms, such as 'molecule', and we say that they obey laws similar to the observed laws of billiard balls, and we connect these by a dictionary similar to the dictionary connecting the billiard ball laws and the mathematical hypothesis; i.e. we say that m, (x, y, z) "are" the mass and position of a molecule.

Let us now turn to the question of other minds. By observation on myself I can obtain three sets of laws which I will call introspective laws (I. laws), psycho-physical laws (P. P. laws), and body laws (B. laws).

An example of an I. law is: An image of X is generally accompanied by an emotional disturbance.

An example of a P. P. law is: My feeling of hunger is generally accompanied by my body taking in food.

An example of a B. law is: My body takes in food at (approximately) regular intervals.

By observation I can also obtain laws which are obeyed by other bodies and I find that these laws are similar to my B. laws when some other body is substituted for my body. From this observed similarity of the B. laws, I can introduce the terms X's mind, Y's mind, etc., which obey laws similar to the observed I. laws and connect these by a "dictionary" similar to the P. P. laws. The following table shows the correspondence between the logical status of "molecules" and "other minds".

Observed behaviour of billiard balls	My observed I. laws
Mathematical laws of billiard balls	My observed B. laws
Dictionary connecting the mathematical laws of billiard balls with the observed laws	My observed P. P. laws
Observed behaviour of gases	Such phenomena as a daily newspaper
"Hypothesis" of the Dynamical Theory of Gases	Other B. laws
Laws of "molecules" (analogous to laws of billiard balls)	Laws of "other minds" (analogous to I. laws)
Dictionary connecting properties of molecules with the hypothesis in the theory of gases	P. P. laws for other bodies.

There are three preliminary comments to be made on this table.

(1) With this analysis it seems we can maintain both that we derive out knowledge of other minds by analogy and that we are certain other bodies have minds, just as we derive our knowledge of molecules by analogy and yet are certain that gases have molecules.

(2) There is a point in which the two cases differ. In the physics case the analogy is between mathematical equations and hence (presumably) is non-empirical; whereas in the psychology case, the analogy is between body laws and is hence empirical.

(3) It is interesting to note that this analysis makes the position of Behaviourism in Psychology analogous to the position of

Mathematics in Physics, and perhaps gives what truth there is in the saying that "Behaviourism brought Science into Psychology".

The use of the table will be made clear by its application to the two following problems. I. The controversy between purposive and non-purposive psychology. II. The psycho-analytic conception of the unconscious.

I. McDougall, for example, argues that much of human behaviour cannot be explained unless we take account of the purposive activity of human beings. And by this he means (at least) that if we are to give an explanation of such a phenomenon as, for example, a daily newspaper, we must take account of the purposes of other men. Moreover by the purposes of other men he means what is ordinarily meant—not just characteristic marks of certain types of behaviour as the purposive behaviourist would hold. The strict behaviourist on the other hand holds that all appeal to purpose is unscientific. Now if we consult the table above we see how this puzzle is resolved. The observed behaviour of gases may be explained by "molecules" or by a mathematical hypothesis and dictionary. For example, if a gas is heated and either the pressure or volume increases, we may explain this by saying that the gain of heat increases the kinetic energy of the molecules and this increases the impulse of the molecules on the walls of the container unless the container expands. Hence either the pressure or the volume of the gas must increase. Alternatively, we may explain it by a statement showing how a change in the value of a mathematical variable occurring in the hypothesis may be translated by the dictionary into a change in the observable relations of pressure and volume. These explanations are not alternative in the sense of one being true and the other false, but are equally admissible.

Consider now the explanation of a phenomenon such as a daily newspaper. Corresponding to the "molecule" explanation there is the explanation by means of other minds; and just as molecules have the properties of perfect elasticity, sphericity, etc., similar to those possessed by billiard balls, so other minds have properties, that is, emotional and purposive experiences

similar to those possessed by my own mind. On the other hand, a different explanation may be given. Corresponding to the hypothesis of the theory of gases there are laws governing other human bodies, for example, that many human bodies enter a newspaper office at 9 a.m. and leave at 5 p.m.; that during that time many human and non-human bodies move in such a way that marks are left on paper, and so on. And just as a dictionary connects a sentence about gases with a sentence in terms of variables, so a "dictionary" could be given that connects a sentence which would ordinarily be said to be about newspaper production with a sentence dealing only with the laws governing human and non-human bodies. There is therefore no question at all as to whether McDougall's theory or that of the non-purposive behaviourist is the correct one, for both are correct.

II. Psycho-analysis was not the first psychological theory to use "the unconscious" but it has done so to a much greater extent than any other, and I think that most of the criticism which has been directed against it is misleading just because it has not realised the logical position. It used to be emphasised in text books on mathematical physics that each step in the mathematical working had an interpretation in the physical world, and the student was advised always to seek "the physical meaning of each equation". Such an attitude is no longer adopted with regard to the majority of theories in mathematical physics. A theory is useful if some of the consequences deduced by pure mathematics from the hypothesis, can be translated by the dictionary into physical terms, but in general there will be many mathematical consequences of the hypothesis which cannot be so translated.

Now the analogous position in the theory of other minds which we are considering would be the following. The mathematical hypothesis corresponds to the laws of bodily behaviour, and the observable laws of the physical world correspond to my observed introspective or inspective laws. Hence those mathematical consequences of the hypothesis to which there is no interpretation in the physical world correspond to bodily behaviour to which there are no inspective states correlated. Now the position of the psycho-analyst, as far as I understand it, is that he recognizes

this, but announces that even if there is no inspective state which is linked to some specific behaviour of my body by means of a psycho-physical law, he is none the less going to say that there is some mental state which is so linked and mental states like this he calls "unconscious". (Of course he does not link an unconscious mental state to *any* bodily act.) Now this seems to me quite sound from a logical point of view. The unconscious, in fact, on this view would have much the same position as the stress in an elastic solid has in the classical theory of elasticity, or the temperature at a point inside a body has in Fourier's Theory of Heat. Neither of these has any "physical reality" in the sense in which forces, or temperatures at the surfaces of bodies have, but it is convenient to consider them ingredients of the physical world, because of certain mathematical theories. Similarly "the unconscious" has not "mental reality" in the sense in which inspective states have, but it is convenient to consider it as an ingredient in the mental world because of certain behavioural laws.

Thus the most common objection to psycho-analysis, namely, that parts of it are unverifiable, rests on the confusion between a law and a theory. In one sense the temperature at a point inside a body is unverifiable, but is a useful conception to have in a theory as it satisfies the desire "to explain" the mathematics. Similarly in one sense my unconscious mental states are unverifiable, but it is a useful conception to have in a theory as it satisfies the desire "to explain" some of the ways in which my body behaves.

As an example, consider the Oedipus Complex. This has been criticized severely on the grounds that it can explain too much, as psycho-analysts (at least those of the Freudian School) have urged that it is part of the normal human mind as well as of those minds exhibiting pathological symptoms. Now it is well known that all four possible combinations of love and hate directed towards father and mother occur among human beings; that is, A may love both his father and mother, B may love his father and hate his mother, C may love his mother and hate his father, D may hate his father and mother. If, therefore, Freud says

that A, B, C, D all have unconscious incestuous desires for their mothers and unconscious desires to kill their fathers, how does this explain the different emotions which these people have?

I think the answer to this is that it does not explain it, but that it was not meant to. The introduction of the unconscious, it must be remembered, was due to the laws according to which bodies behaved and was not due to the emotions or other inspective states which minds experienced. Thus "the unconscious" is used in such a way that it is quite possible for one person to be experiencing the emotion of love towards another and at the same time to have an unconscious hatred towards him. It is therefore quite possible for everyone to have an Oedipus Complex if the behavioural acts of individuals towards their parents are uniform in certain general ways, and psycho-analysts maintain presumably that this is so.

Moreover it is possible that not only should conscious and unconscious desires be opposed but also that there should be opposing unconscious desires in the same mind, and this is sometimes supposed to be the crowning absurdity of psycho-analysis. For example, Flugel in an article "The Psychology of Birth Control" (J. C. Flugel, *Men and their Motives*, pp. 1–43) argues that the doctrine of birth control is unwelcome to some people both because it threatens the active manifestations of the sexual tendencies and because it threatens the repressions thereof. "It is repellent, not only because it reveals the necessity for sexual inhibition, but also because it is too intimately concerned with sexual matters . . . and appears indeed in some respects to open up greater possibilities of sexual freedom".[1] Such an attitude is defended by the psycho-analysts by saying that the human mind, especially the unconscious, is fundamentally illogical. But that seems to me rather misleading as the unconscious can be neither logical nor illogical. All that is necessary to resolve the paradox is to realise that the same human bodies at one time manifest great sexual liberty and at another time the reverse, and that it is ultimately such behaviour which gives a meaning to opposing unconscious desires.

[1] Op. cit., p. 31.

Science: Natural and Social

BY

THOMAS I. COOK

I

THE problem of what constitutes science is of considerable significance for the student of society: his work, both in its methods and its results, so far as it claims to be scientific, is regarded sceptically. Possibly in consequence he has tended recently to support a broad definition of science which identifies it with knowledge. Yet, leaving aside the difficult problems of what knowledge is, or what it is knowledge of, most of us would argue that, while knowledge may either be derived from science or lead to science, the two are not equivalent.

Science would seem to imply not merely a body of facts, but a homogeneous body as against other facts seen to be irrelevant. However, facts once regarded as not relevant to one another may, as we see relationships that have not been seen before and realize that such relationships are not casual or chance ones, gain relevance and may be put together into something we perceive to be a science. This seems to require their systematic arrangement in terms of continuing uniformities that allow the formulation of laws which help to interpret the universe, or some section of it. In the sense that, ideally, such laws must cover all facts admitted to be within their sphere, not simply facts hitherto observed, but facts of a like kind which emerge in the future, science is said to be characterized by predictability. Yet it can

do more than explain relationships only so far as understanding of them may enable us to create controls of parts of our environment, or to adapt ourselves to circumstances beyond our control.

Utility, however, cannot be accepted unreservedly as a test of science. There seems no *a priori* reason for assuming that all knowledge of a scientific character will, in the ordinary sense, be useful, immediately or even ultimately, if only because we lack the knowledge necessary to see the practical implications. Science is always useful, on the other hand, as a system of rules which satisfies the desire for relating facts and ordering our universe; and this may constitute an acceptable definition if we insist that not only must the desire be satisfied, but that the rules formulated do cover, and correspond to, the facts observed.

II

Stress on method or confusions as to its meaning have, nevertheless, obscured the nature of science. Observation, hypotheses, and verification, it is said, must be undertaken in their proper order to arrive at a law that will be final unless and until further observation reveals exceptions calling for a new and more comprehensive hypothesis. But the orthodox procedures supposedly basic to scientific knowledge seem generally to be *ex post facto* formulations of what might have been done, but was not in fact done; of what, psychologically, it might be very nearly impossible for a scientist to do. An admirable means of testing laws already known, they offer very little as guides to new knowledge. Certainly they are not means to major discoveries which necessarily involve breaks with existing knowledge.

In a sense, then, science is reduced to a mystery in which any major development, however dependent on minor collaborative research, proceeds from inexplicable genius that perceives relationships not generally visible to others. Hence it appears unwise to argue that the essence of science is the validity of a method of thinking, a technique of experimenting, and certain means of verifying, save in the specific sense that we must insist that scientific laws, when obtained, must be capable of being checked by methods that we are prepared to recognize as adequate and valid.

III

Science is at times said to depend on the concept of a rational order. Whether this necessarily implies a complete pattern of the universe at the start and through time, parts of which are gradually, and perhaps not systematically, revealed to the human mind, is not entirely easy to answer. Modern thought has, indeed, complicated that question. It conceives of a universe itself evolving and developing in terms not of the consistent pattern of the Newtonian world, but of laws of growth and change; laws discovered and interpreted by human beings who are themselves subject to them and parts of the process. On the one hand, we talk of a scientific order subject to law, on the other hand we argue that that order is a continuous process involving growth and change.

This whole paradox gains importance when we realize that, connected with the idea of rationality as basic to science, there are the twin concepts of causality and uniformity. Both imply that random events do not occur, while they further lead to an assumption, seemingly necessary for science, that all events could be explained fully if we knew all the facts, and consequently to a theory of determinism. But paradoxically we see the emergence of novelty. Scientific knowledge appears to depend on certain laws, yet in its conclusions to contradict their validity.

Such conclusions may be unacceptable. Yet doctrines consonant with the observed facts of arbitrary non-predictable behavior, whether of vitalism or of emergent evolution, leave unsolved the question of how the development itself may be at once caused and undetermined. Any theory that would find the element of novelty in chance meetings of parts themselves caused, would, in the last analysis, break down in that one could not establish any genuine cause for parts without its being part of the cause for the whole. But the doctrines of rationality, causality, and uniformity, are essentially doctrines of an ultimate unity.

IV

Sometimes, however, it is claimed that science is simply concerned with generalized description, with laws of functioning,

by contrast with the medieval scholastic scheme of the universe, which was concerned always with purposes and reduced the world to illustration of moral principles and of God's will. Such a contrast is, however, much too facile. True, we have succeeded in divorcing human purposes and social values from the processes of a universe which we can control only by understanding, and not by reason of its conformity to our norms.

Yet this hardly justifies the conclusion that we are not concerned with why. Indeed, the assumption that it is only how that is significant arose largely from the mechanistic interpretation of the Newtonian scheme, when time was held to be irrelevant to a continuous and identical process. But modern science has restored time as an essential element in the workings of the universe. Hence laws cease to be mere descriptions of how forces operate with continuous repetition and become, at the very least, descriptions of how they operate developmentally.

When we discuss the how of a dynamic universe, we come ultimately to questions of origins, and if we can no longer talk of a *primum mobile*, in the sense either of the Newtonian clock maker or of the medieval God, our descriptive rules of development soon force us back to the limits of human intelligence as Kant saw them. Now, it may be that the modern integration of time and space has itself rendered the old question of beginnings irrelevant by making time and space inherent attributes of one another and by putting us once more in a closed world, finite though vast. Nevertheless, though this might seem to leave us with the sole question of how, now made a proper form of approach rather than an ignoring of an integral element in the question of methods of operation of the universe, there is still no complete agreement on whether that universe is finite, while the structure of our minds seems to drive us always to the question of why, to the discovery of assumptions that are metaphysical rather than physical.

Moreover, questions of epistemology arise repeatedly to take us back, in due course, to that ancient question of the reality of the external world and of our intelligence's relation to it. Indeed, the work of Einstein seems peculiarly significant in this respect. For Einstein has apparently attacked the Newtonian theory on

the ground that it is relativistic and anthropocentric (as against the absolutistic anthropocentricism of medievalism), and has discovered laws of the universe that are freed from the error of observation conducted from a particular point in space and time, while at the same time covering observations of a spatial-temporal universe and not going outside it for an absolute. He has, that is to say, while in particular location of space and time, somehow transcended it by the use of reason.

That, however, simply raises the problem of whether a finite being in a particular location can genuinely achieve this emancipation, and, while experiments seem to have confirmed his findings, they too are made by human beings particularly situated in the universe. The essence of such discovery seems to be that the one constant in our universe is the speed of light. One is driven thereby to a curious world in which light becomes God, the measure of all things. This might lead to a new faith in a perfected closed universe with the question of how answered or, to put it differently, identified with the question why, not by an active faith resting on a false imputation of human moral purpose to the universe, but on a knowledge that the universe did inevitably work in a certain way with one uniformity as its measure. The why would be answered not by reading it out of court as irrelevant, but by the establishment of fact which explained how and was the ultimate measure of existence. Unfortunately, however, modern physics seems also to have discovered, and as a part of the same growth that is characterized by Einstein's law, that reason and experiment show indeterminacy in the universe, and as a consequence to have convicted us, if not of ignorance, then at least of limitation.

V

The essential contrast between the scientific analysis of the universe and the theological or ethical is not, we may therefore infer, in the contrast between how and why, but in the contrast of these with the question of ought. The question of why has become more basic, rather than less so, by the rejection of measurement of the universe by man's values. The fact that scien-

tists have continuously rejected specific patterns as inadequate because of conflicts with facts has not led to abandonment of the idea of pattern, but simply to patterns more difficult to create. In this sense it is urged that the characteristic of scientific development is the growth of larger and larger systems, covering more and more facts under single laws. This may perhaps be taken to imply that when and if science itself became perfect, all knowledge would be the consequent of one great law; and that idea is successful as a pragmatic approach.

Thus we are brought once more back to the problem of the connection between human reason and the external world. Moreover, to the student of ethics, at least, the criterion of harmony, which is a rational ideal that men pursue but do not achieve, involves the same kind of assumption as that of the scientist; and, while we cannot assume that the world achieves our moral values, we are nevertheless driven to raise the whole question of the relation between rational norms and a rational pattern for the universe.

This whole series of problems is made the more confusing when we note that our grasp of patterns in both spheres is imperfect; yet that in both such patterns enable us to develop new knowledge and to proceed in creating control of our environment or of our fellows. Once more we get back to the curious problem of imperfection as a road to perfection.

Nor is this all. Both in science and in social theory there seems to be a pluralism of patterns equally rational, suited each for different purposes, but each leading to effective results in its own spheres. Now whether these are different ways of stating the same conclusions, whether there is an ideal form of statement to which others are imperfect approximations, or whether all of them are approximations to a hitherto undiscovered pattern is not clear. Yet the reason which we use as a tool would seem in itself to imply a perfect single pattern, to which a variety of patterns, all good and all useful, lead.

Yet again individual sciences and studies, normative or descriptive, necessarily involve an abstracting from the totality of events or an ignoring of certain aspects of things in order to

systematize some area. Acknowledged to be sound and necessary, this leads to the escape of the whole. Yet its purpose always is to bring about rules that shall be part of a total rational design, and the perception of facts that upset specific generalizations of particular sciences usually leads to a reformulation that tends to integrate sciences.

To study, we must abstract and define. But, as our abstractions and definitions are successful, they break down and integrate with other similar abstractions, themselves likewise achieved. Now ultimately the aim seems to be to achieve a generalized description that will cover the totality of events and relationships, will explain all, and yet will be the product of a whole series of acts of abstracting and synthesizing. To explain phenomena and to create a science, we have to ignore as irrelevant certain aspects of the material studied. Out of this process long maintained, we get, finally, laws which transcend the particular sciences and put everything back together again, so that ideally we know and explain the whole by a formula or series of formulae which have grown from an admitted falsification for purposes of study. Finally, one has the whole question of what it is that is explained and embraced by such a formula and what is its real relation to us or to the processes studied.

VI

For the social scientist there emerge from the foregoing analysis certain inferences which, while general rather than precise and negative rather than positive, have nevertheless considerable significance. First of all, the social scientist's profound respect for the natural sciences seems to rest on certain false conceptions. He has been impressed by their supposed possession of watertight methods for achieving knowledge. This is in part due to their insistence that hypotheses and interpretations must be tested and must cover new observations to which they are supposed to apply. It is due no less to the use of method to achieve practically useful applications of such principles from the viewpoint of control of environment and the extension of creature comforts. The social scientist has failed to note the importance

of major jumps in the development of knowledge and to perceive that these are made on the basis of a perception or intuition not unlike that which he himself so frequently employs. Science is indeed pragmatic in the sense that its conclusions have to conform to facts of observation. The truth is for it what works, but 'work' means simply 'correspond with data.' The scientist readily throws overboard fine theories when they fail to work in this respect, and here the student of society is no doubt justified in emulating him. Unfortunately, however, the latter's belief that a true science must be testable has led him to a conviction that the pragmatic is the practically useful, as well as to the curious notion that facts are both beginning and end, that hypotheses must be avoided, and that the attempt to formulate theories which will bring together under simple laws the complexities of divergent phenomena is a work of folly and of supererogation. Yet if anything emerges from a study of science it is that its essential discoveries have arisen from a lack of concern for the practical and that the most useful consequences have generally developed indirectly, unsuspectedly, and frequently after a considerable lapse of time. Moreover, while the testing of the principles propounded by the scientist is pragmatic, the achievement thereof arises from attempts to explain and embrace under simple laws, and such attempts generally have no connection with the pragmatic. Indeed pragmatism in science, as properly also in philosophy, is rather a curb on, and a method of choice between the constructs of imagination and of reason than a point of departure or a foundation for knowledge.

If the social scientist is impressed by the methods and by the pragmatic nature of science, he is at the same time overcome with awe at its systematic character, its coherence, its pattern. Submerged by the complexities of the events which he has to study, he admires the reduction of the external world to order and to law by his confrère. He fails, however, to observe three things: first, that such law involves transcending the facts; secondly, that the scientist is continuously demolishing the order created that he may achieve a more comprehensive order; and thirdly, that in doing this he does not totally reject as totally false what

has formerly been accepted, but sooner or later makes of it a special part of his enlarged system. The social scientist, on the other hand, very usually throws overboard entirely any theory of explanation when he finds that it is inadequate to explain new phenomena which come within his ken but which were previously unknown. Instead of incorporating theories that explain areas of existence but do not comprehend the whole, he tends to reject them as entirely untrue. The result has been a total distrust of all explanation, as was suggested above; and this, because of a misunderstanding of scientific procedure, is believed to be the very essence of science.

Thirdly, however, it is to be noted that the social scientist, recognizing that the unity of knowledge is an ultimate, a guiding postulate, and not something achieved or to be continuously pursued in the process of reducing phenomena to law, has of recent times become professedly sceptical of borrowing the conclusions of the pure sciences as the premises of his own. He has seen how in the past the idea of natural law, drawn from the pure sciences, played havoc with his own discipline. He has realized the follies committed in the name of reason by Locke and his successors through their attempt to use the triumphs of Newton. Finally, he has been aware of the evils arising from false analogies from Darwin and the doctrine of evolution. A unity of spheres is not to be produced by the short cut of facile analogy.

Nevertheless, even those who profess such scepticism not infrequently do borrow from the current reigning ideas. In particular, relativity is the rage. Social scientists, seeing some connection between a physical law that they do not understand and the sociologist's stress on the particularity of cultures, have become worshippers of a relativism that ends in a lack of any set point of departure; and this has also in some mysterious way become associated with the theory of the pragmatic approach. The error is no doubt largely a consequence of verbal similarity, due to the seeming implication of the term 'relative.' There has been a total failure to realize that Einstein's doctrine was the product of a search for an absolute and a criticism of Newtonianism just because it was relativistic. Yet the social scientist has

used this explanation of the universe to condemn all theoretical analysis, and particularly that type of analysis that endeavors to discover some valid criterion of judgment beyond the norms and mores of specific societies.

Such an attitude has been further bolstered by the very different theory of indeterminacy. This scientific doctrine, which applies only to those ultimate and uniform individuals of the natural world, and not to masses of them, and which seemingly has no corresponding individuals, at least hitherto known, in the world of society, has been generalized by the social scientist into a glorification of probability as the essence of the universe; and this in turn has led to a worship of mathematics and of statistical technique as alone producing any worth-while interpretation. Yet, while doubtless the discovery of such individuals has been one of the triumphs of modern science, as their peculiar behavior has been one of its most curious puzzles, a very considerable part of scientific knowledge is still knowledge of masses, and not of individuals; and these masses seem to be subject to laws that are something more than laws of probability.

Yet perhaps the worst consequence of the impact of relativity theory and of the indeterminacy principle in the world of the social scientist has been that these have combined with the findings of modern psychology as to the non-rational elements in man to lead to a strange belief, not simply that behavior and the universe alike are irrational, but that reason itself is a useless tool. Thus science, which, whatever the paradoxes involved in its pursuit, has depended on and had faith in reason as a necessary tool, itself has been employed, subtly and unwittingly, in the support of the revolt against reason; and this not infrequently by those who avowedly condemn the extreme forms of political anti-rationalism on various grounds, of which their acknowledged conflict with the scientific spirit is by no means the least important.

University of Washington, Seattle, Washington.

The Place of Theory in Anthropological Studies

BY

CLYDE KLUCKHOHN

IT IS probably true that the greater number of contemporary American anthropologists feel that "theory" is a very dangerous kind of business which the careful anthropologist must be on his guard against. This statement represents, in the first instance, merely a crude induction from my experience in talking with professional anthropologists. It is, however, symptomatic that not until 1933 did a book by an American anthropologist include the word "theory" in its title.[1] Only a single book published subsequently is explicitly given over to anthropological theory, and this avowedly concentrates upon the historical development of theories rather than upon a fresh and extended analysis of the more abstract aspects of anthropological thought.[2] But because anthropology still painfully remembers the stomach-ache it got from the too easy generalizations of many nineteenth century "arm-chair ethnologists" is insufficient reason, it seems to me, for that almost morbid avoidance of theory which tends to produce acute indigestion from sheer bulk of unordered concrete observations. Landheer has with some justice commented:

[1] Paul Radin, *The Method and Theory of Ethnology* (New York, 1933).

[2] R. H. Lowie, *History of Ethnological Theory* (New York, 1937). (It is convenient to confine the discussion to the work of American anthropologists, and others will be mentioned only incidentally.)

"On the other hand, and this is especially true for America, we find scientists occupied with accumulating facts without being very certain about the concepts which could give to these data a scientific significance."[3]

My impression that American anthropologists (in spite of some evidence of a reawakening of theoretical interest during the past five years) are still devoting an overwhelming proportion of their energies to the accumulation of facts seems confirmed by the following empirical test samples. I examined all articles which have appeared in the *American Anthropologist*, the *American Journal of Physical Anthropology*, and *American Antiquity* (Journal of the Society for American Archaeology) since January 1st, 1935. In the first-named journal I found only fourteen articles (out of 152) which were not essentially exclusively descriptive in nature. Seven of these (such as Hawley's "Pueblo Social Organization as a Lead to Pueblo History," Opler's "Apache Data Concerning the Relation of Kinship Terminology to Social Classification," and Li Anche's "Zuni: Some Observations and Queries") were basically examinations of particular assemblages of fact in terms of principles which were taken as given. Only seven articles out of 152 were devoted to theory in the sense of discussion of the canons of reasoning in anthropological procedures. In the *American Journal of Physical Anthropology* but one article in ninety-eight was concerned with theory (I did not count, of course, papers on mechanical techniques). In *American Antiquity* four articles out of sixty-eight (including such contributions as Gillin's "A Method for the Description and Comparison of Southwestern Pottery Sherds by Formula") departed somewhat from sheer description, but only one (Steward and Setzler's "Function and Configuration in Archaeology") considered theory if we equate theory with "conceptual scheme." I also classified on this basis the books and papers listed in the "Some New Publications" department of the *American Antropologist* since January 1st, 1935. Out of 1992 which could be ex-

[3] B. Landheer, *Presupposition in the Social Sciences* (American Journal of Sociology, vol. 37, 1932, pp. 539–546).

amined in the library of the Peabody Museum of Harvard University (or where an almost certainly correct determination could be made by title) eighty-eight appeared to admit some theoretical element, but this total was decidedly increased by the inclusion of works by sociologists and of a disproportionate number of titles by European anthropologists.

In any case the alternative is not, I think, between theory and no theory or a minimum of theory, but between adequate and inadequate theories, and, even more important, between theories, the postulates and propositions of which are conscious and which hence lend themselves to systematic criticism, and theories, the premises of which have not been examined even by their formulators. For I am afraid that many of our anthropologists who are most distrustful of "theory" are like Moliere's character who spoke prose without knowing it, for a complex theoretical viewpoint is usually implicit in some of the most apparently innocent "statements of fact." The very tables of contents of many recent and standard ethnological monographs tend to beg complex questions. To what extent do the headings "Material Culture" "Religious Life" "Economic Life" and the like represent categories which have arisen out of a purely inductive analysis of the raw data? To what extent do they reflect a distortion of data from their context, a formalized and traditional dismemberment of the facts as observed?

At most, only the first task of scientific research (that of pure description of concrete phenomena) can be performed independently of theory. Indeed, even this statement may properly be questioned, since simple description[4] necessarily involves selection out of the vast amorphous body of sense data which impinges upon the consciousness of the observer. Poincaré has sagely

[4] I am aware that some philosophers of science maintain that all science is (or ought to be) description. But in so doing they inflate considerably the ordinary extension of the concept—specifically, they include the ordering and analysis of data, as well as the cataloguing of characteristics of separate percepts. To put the matter somewhat differently they urge that science must concern itself with only the first two of Aristotle's causes—must only attempt to answer the questions "what?" and "how?" I employ the word "description" in the narrower sense—not including the establishment of relations between data, other than those given in the process of primary perception.

observed "... la methode c'est precisement le choix des faits."[5] And Malinowski says:

"description cannot properly be separated from explanation, since in the words of a great physicist 'explanation is nothing but condensed description.' Every observer should ruthlessly banish from his work conjecture, preconceived assumptions, and hypothetical schemes, but not theory."[6]

The dichotomy between "fact" and "theory" is often a useful one, and very often when we use it we no doubt know approximately what we mean and so does the recipient of our statement. But, apart from the convenience of such rough pragmatic usage, the sharp distinction has by no means an unquestioned philosophical or logical justification. Kant has vigorously maintained that many percepts and concepts have subjective and objective meaning only in terms of categories which are by no means altogether empirically derived. In this fundamental position (insofar as its logical—but not necessarily its metaphysical—implications are concerned) he has been followed by many subsequent philosophers of different schools. Joseph has remarked:

"But there can be no purely descriptive science, for the simplest description of what we perceive makes it coherent by connecting it with something not perceived."[7]

And Whitehead says:

"... the first point to remember is that the observational order is invariably interpreted in terms of the concepts supplied by the conceptual order."[8]

I would not wish to press this philosophical point of view too far, for undoubtedly in a pragmatic way we can and must distinguish on occasion between discrete percepts which in some sense at

[5] Poincaré, Science et Methode, (Paris, 1909) p. 12.

[6] B. Malinowski, Article, *Social Anthropology*, (Encyclopaedia Britannica, 14th edition, vol. 20) p. 864.

[7] H. W. B. Joseph, *Essays in Ancient and Modern Philosophy* (Oxford, 1935), p. 31.

[8] A. N. Whitehead, *Adventures of Ideas* (New York, 1933), p. 198.

least are directly verifiable empirically and abstract concepts which do not lend themselves to such immediate verification. One can see whether an axe is three-quarter grooved or full-grooved but no one has ever seen a "cultural configuration" in this immediate and direct way. The operations by which such concepts are arrived at, by which they may be inferred from what Bertrand Russell calls "hard-sense data" must, of course, be scrupulously and plainly specified. Indeed I feel that Stace correctly says:

> "... a concept without application in experience is meaningless; and this truth has to be admitted, whether we take an empiricist or a rationalistic view of the genesis of concepts."[9]

But the differing philosophical views of the matter are worth bearing in mind as a corrective against the ingenuous view that "fact" and "theory" are always distinct and easily separable categories. As Parsons points out, [10] if science consisted merely of facts, there would be no "crucial experiments."

In my opinion, not many American anthropologists, if pinned down, would agree without qualification with Radin's rather recent statement:

"The only question of importance, then, is to discover some means whereby we can best obtain a complete account of an aboriginal culture."[11] Nevertheless their behavior, in general, strongly suggests actual passive acquiesence in this point of view. It is not merely that they devote themselves, for the most part, principally to pure description (with some forays into interpretations based upon distributional surveys). In addition, a certain suspicion very generally attaches in anthropological circles to those few anthropologists like Kroeber and Wallis who have given sustained attention to theoretical questions. The eminence of Kroeber, for example, may be granted, but (at least in informal

[9] W. T. Stace, *Metaphysics and Meaning* (Mind, vol. 34, pp. 417–439), p. 422.

[10] Talcott Parsons, *The Structure of Social Action* (New York, 1937), p. 8. I should like to acknowledge at this point my great general debt to my colleague, Professor Parsons, with regard to my thinking on the topics on which this paper touches.

[11] Radin, *op. cit.*, p. xi.

conversations) serious reservations are expressed which appear to me to be based at least as much upon an intrenched resistance to "theory" as upon conviction that certain of Kroeber's conclusions are not congruent with the relevant empirical data. It is difficult to document these assertions of mine by other than the device of anecdote and the method of highly imperfect induction. I must simply record my honest impression (and those anthropologists whom I have specifically questioned on the point agreed that it was theirs also) that the very word "theory" has a pejoratif connotation for most American anthropologists. To suggest that something is "theoretical" is to suggest that it is slightly indecent. "Theory" indeed tends to be roughly equated with "speculation". This is of a piece with the too prevalent tendency to assume tacitly a kind of antimony between "facts" and "theory." Even Strong whom the writer would decidedly group with the minority who give evidence of awareness of the necessity for theoretical discussion has recently quoted with seeming approval Laufer's observation "We should all be more enthusiastic about new facts than about methods. . . ."[12]

Closely bound up with the opinion that a relatively simple first principle of anthropological research is to seek "facts" and avoid "theories" is the equally misleading view that "common sense" is more trustworthy than "theory." It is urged in effect that, if more than the amassing of "facts" be granted as desirable, "common sense" is the only safe and all-sufficient guide in the ordering and interpretations of Data. Of this *sancta simplicitas* one can only say what Joseph has said in a slightly different context:

> "Now in all this I am disposed to believe that there is much loose thinking, and many problems are overlooked under the hypnotic influence of a blessed word."[13]

In the first place, the history of thought has given us many dramatic instances of the inadequacy and deceptiveness of

[12] W. D. Strong, *Anthropological Theory and Archaeological Fact*, (*Essays in Anthropology in Honor of Alfred Louis Kroeber*, Berkeley, 1936, pp. 359–371), p. 368.

[13] Joseph (*op. cit.*), p. 304.

"common sense" as applied to scientific problems. "Common sense" blinded the minds of many generations of men to the possibility that the world could be other than flat. "Common sense" was perfectly certain that swamps were the sole cause of malaria since malaria occurred in swamp-infested regions. In the second place (and this of course is the decisive point!) "common sense" itself is far from being free from the taint of theory. As Bloomfield has acutely remarked:

"... much ... that masquerades as common sense is in fact highly sophisticated and derives, at no great distance, from the speculations of ancient and medieval philosophers."[14]

In short, insofar as science is concerned "common sense" is simply another name for "theory," but it is bad theory, for its structure is so much beneath the ground of consciousness that unwarranted doctrines tend to be uncritically accepted. The anthropologist should have been fully aware of the fraudulence of the "common sense view," for "common sense" is so obviously a cultural product. Therefore an extreme degree of relativity is involved. In other words "common sense" represents the rather crude and generalized "theory" which the average intelligent person of a particular generation in a particular culture applies to experience in general. Hence "common sense" even more than theories in general has a devastatingly slight constancy. But science must aim, at least, at theoretical principles which are more universal and which more nearly approach absolute validity.

To sum up the argument to this point: science is on the quest of knowledge as well as of information, hence it is a form of intellectual cowardice to maintain or imply that we should stop with the accumulation of "facts" simply because their interpretation is fraught with difficulties and perils and has in the past led anthropologists to positions which have subsequently been shown to be absurd. And it is a form of intellectual naïveté to believe that anthropology could dispense with theory if it would. Fi-

[14] L. Bloomfield, *Language* (New York, 1933), p. 3.

nally, it is a dangerous form of intellectual slovenliness to suggest that "common sense" is a preferable alternative to "theory."

It seems highly desirable that *every* anthropologist should have some grasp of the assumptions overt and covert involved in the more important methods of attack on anthropological problems in use in his own time. For it is important not merely to read what anthropologists say but also to endeavor to understand what they mean—which is often (unhappily, but probably inevitably) an additional, separate task. The precise significance of the utterances which an anthropologist makes (after his laborious researches) emerges sometimes only if we are able to view them as placed firmly in the context of his theoretical viewpoints. Otherwise we are all too likely to react to what are, for us, only verbalizations by equally unmeaningful speech reactions.

But the approaches of many anthropologists have to be minutely dissected out. Only thus are the suppressed premises (which are fundamental to the *understanding* of all that they have written) revealed and subject to dispassionate study. Only when the underlying presuppositions have been made distinct do the limitations involved become evident. There is great need, I feel sure, for constant critical reëxamination of the postulates basic to the several aspects of anthropological studies. Therefore, it seems justifiable that *some* anthropologists should devote some part of their research time budgets to an intensive study of these theoretical approaches and their relations to the wider horizons of thought. Such anthropologists might well at the same time occupy themselves with that finer discrimination of conceptual detail which must keep pace with the accumulation of discrete empirical observations, if anthropological research is not to be largely sterile.

The extent to which the theoretical structures of American anthropologists have lacked explicit statement (and hence susceptibility to critical examination) is well illustrated in the case of Boas, long the outstanding figure in the field. Boas permitted himself one brief, programmatic article on "the Methods of Ethnology"[15] in which he stated certain problems and made some

[15] *American Anthropologist*, vol. 22, 1920, pp. 311–321.

astute comments on what seemed to him fallacies in contemporary approaches. Elsewhere in his writings he has made numerous observations of a theoretical nature which are in themselves most useful and illuminating. But nowhere has he given us a coherent, detailed, systematic presentation of his theory. Lowie speaks of Boas' "deliberate aversion to systematization."[16] Certainly, at least until Lowie's recent discussion,[17] one has had to turn to MacIver, a sociologist, for a succinct statement of Boas' theoretical viewpoint.[18] In some part historical factors (the attitude of Boas, his dominance, the general relationships of anthropologists in this country) explain the American situation. Until very recently all but a few of the leading anthropologists in the United States had been trained by Boas or at any rate come under his personal influence. Moreover, the number of professional anthropologists was small enough so that essentially all of them knew each other personally, and a great many matters of theory were, in effect, settled by personal and informal conversations which were never reported on the printed page. At least until quite lately one could, with little violence to the facts, speak of a single dominant tradition in American anthropology, the tradition of Boas. The degree to which this was unformalized and yet realized is reflected in this story: A young anthropologist, trained largely in Europe, wrote an article in which he criticized some of the theories apparently held by the "Boas school." A well-known older anthropologist, himself a pupil of Boas, commented "You are perfectly correct so far as the written record goes. The trouble is that we have never bothered to write down some of our most basic methodological principles. We all know each other. We talk things over. If one of us writes something which Boas or some of the rest of us feel is contrary to the canons which all of us—more or less unconsciously—accept, he hears about it and the thing is threshed out."

To say that "the study of the concepts of a discipline as such is of value" is an understatement. It is of quite indispensable

[16] *Op. cit.*, p. 152.
[17] *Ibid.*, pp. 128–155.
[18] In: *The Encyclopaedia of the Social Sciences*, vol. I, p. 185.

importance to any science. The view of Radcliffe-Brown (and many others) that criticism in science should always consist merely "in a re-examination of the evidence adduced in favor of a hypothesis"[19] I can only regard as grossly fallacious. This verificational aspect of scientific procedure is essential, of course. But equally needful is the critical and systematic study of the family of presumptions from which the re-examination of the evidence proceeds, and of the categories in terms of which it is carried out. The nature of scientific procedure inescapably has these two separable aspects.

It is true, of course, that Radcliffe-Brown, for example, recognizes the inevitability of "theory" and "hypotheses" in anthropology. He says, indeed:

> "The procedure in our science must therefore rest on the building up of a body of theories or hypotheses relating to all aspects of culture or social life and the testing of these hypotheses by intensive field research."[20]

This at first sight may seem to indicate a position practically identical with my own. But I think I can show that this is not the case. I agree with all that Radcliffe-Brown says, but I insist that he does not go far enough. I maintain that his view[21] (which may be considered as roughly the type of that of most anthropologists who will traffic with "theory" at all) implies or assumes that the "theories" and "hypotheses" arise directly out of the discrete data of anthropology, in terms of which they must again be "verified, rejected, or modified." This seems to me a regrettably naïve judgment, for it can easily be shown both that the concepts applied have in few cases emerged out of anthropological "facts" and that the axioms of general scientific procedure have certainly been taken over from other studies. Are these two classes of ideas which assuredly enter into the end-

[19] A. R. Radcliffe-Brown, *The Present Position of Anthropological Studies* (Report of the British Association for the Advancement of Science, London, 1931, pp. 141–171), p. 150.

[20] *Ibid.*, p. 157.

[21] Which he reiterates both directly and inferentially time without number in the work referred to and in other places.

products of anthropological research to be accepted as sacrosanct dogmas in a kind of scientific fundamentalism? Are they to be immune from that disinterested microscopical dissection and macroscopical synthesis which is veritably the life of all science? I think not. On the contrary, a strong brief can be advanced for holding that in anthropological studies at the present time the need for attention to what Rice calls the "conceptual version" of method is more pressing than to the observational phase of method (which is already relatively well developed, particularly, perhaps, in the United States).

Our techniques of observing and recording are admittedly still susceptible of improvement, but they seem much further advanced than our development of symbols (verbal and otherwise) by which we could communicate to each other (without loss or inflation of content) the signs and symptoms we observe. In archaeology, for example, methods for classifying pottery wares on the basis of highly technical and rather precisely defined operations have been elaborated. But I am aware of but a single paper[22] (by a Russian!) where there has been even a tentative and fumbling consideration of the implications of the typological method. Such archaeologists as Vaillant, Strong, Setzler, Gladwin, and Paul Martin are (but only very recently) evidencing searchings of their theoretical consciences, and this is a happy omen. Meanwhile typologies are proliferated without apparent concern as to what the concepts involved are likely to mean when reduced to concrete human behaviors. The status of such concepts as "race" and the lack of statistical validation for the various indices remain a scandal to physical anthropology. In social anthropology and ethnology such terms as "pattern" and "configuration" are used with reckless abandon. The same anthropologists sometimes appear to mean by pattern "norms" or those responses to given situations which the ideology of a particular culture recognizes as ideal, sometimes the culturally stylized responses which in terms of actually observed behaviors represent something like the statistical mean or mode; occasion-

[22] V. A. Gorodzov, *The Typological Method in Archaeology* (*American Anthropologist*, vol. 35, 1933, pp. 95–103).

ally the usage vacillates between these two referents. Often "pattern" and "configuration" are used as approximate equivalents, perhaps equally often "configuration" refers to a more general principle in terms of which the numerous discrete patterns tend to be organized.[23] Linton's "Study of Man"[24] seems to me to stand almost alone in recent anthropological literature as a resolute attempt to establish fairly exact referents for concepts and to introduce new and needed concepts.

Examination of the evidence bearing upon a hypothesis or upon the statement of a putative uniformity is of absolutely critical importance. No one wishes to deny this. Sociology has with little question suffered from an over-elaboration of the theoretical aspects of the discipline, and I am far from wishing anthropology to abandon a tradition which is weighted on the empirical side. My contention is only that the weighting has become somewhat unbalanced. Such theory as there is has, with rare exceptions, been incidental to monographs on particular peoples. Now it is desirable, certainly, that abstract concepts should be illustrated by particulars and their utility tested by applying them to a given assemblage of facts. But often one senses that it is primarily a case of allowing a little theory to sneak in the back door—as if anthropologists were half ashamed to discuss their logics in the open. Moreover, it is sometimes hard to disentangle the theory from the particular culture and society—how far are the axioms and postulates thought of as having broad reference? There are critiques of limited problems like that of diffusion, but almost no attempts to define basic concepts operationally. And yet experience seems to show that two fairly distinct operations are prerequisite to substantial advances in scientific knowledge: (1) observation, (2) reasoning. These are in no sense mutually exclusive alternatives, and any

[23] For a preliminary exploration of the concept "configuration" as used in anthropology see John Gillin, *The Configuration Problem in Culture*, (American Sociological Review, vol. 1, 1936, pp. 373–387). It is not, I think, without meaning that this and other papers on theory by anthropologists (such as Kroeber, Wallis, Thurnwald) which have appeared during the past few years have been published in sociological rather than anthropological journals.

[24] New York, 1936.

attempt so to distort the issue must be resisted. They supplement and complement each other. Any sound intellectual structure must be based, as the dialectical materialists have urged, upon a blending of practice and theory. A plethora of observations are at present at the disposal of anthropologists, but at present these observations have been but little synthesized[25] or conceptualized. Most of all, anthropological literature is singularly deficient in critical discussions of the proper method of reasoning.

There are, I maintain, at least two aspects of scientific procedure which can be disregarded only at great cost to the significance of scientific investigation. The first is the consistency and logical justification of the abstract concepts of a discipline. The second is the relation of these abstract concepts to other systematized bodies of knowledge. American anthropological thought has been unusually parochial. And yet I think we have on every hand demonstration of the correctness of Professor Whitehead's conviction that "celibacy of the intellect" is the most insidious threat to modern intellectual advance. As Whitehead says, "each profession makes progress, but it is progress in its own groove." Each profession tends to contemplate a single limited set of abstractions. Hence its categories are very imperfect. Emphases in anthropological field work, for example, have tended, as Mead has pointed out, to continue along purely traditional lines—which were themselves established somewhat accidentally. As she suggests, virtually every ethnographer makes observations on such topics as circumcision and the disposal of the umbilical cord but few give details on the manner of weaning or on the position a child is held in while being suckled.[26] Ex-

[25] It is noteworthy in anthropology at the moment that (with a few noteworthy exceptions) one must leap from the minutiae of monographic studies to semi-popular or popular books. The literature on the ethnology of the North American Indians, for example, has reached tremendous proportions, but there is simply no professionally acceptable synthesis of the German "Handbuch" type. The comparative lack of synthetic "library research" in anthropology is, of course, a separate question from that which is here the centre of our interest, but it is clearly very closely related to certain rather general attitudes which seem to me manifestations of the "occupational psychosis" of anthropologists.

[26] Margaret Mead, *More Comprehensive Field Methods* (*American Anthropologist*, vol. 35, 1933, pp. 1–16).

perience seems to show that reading in psychology, sociology, statistics, and other fields tends to spur anthropologists off the too well-beaten trails of investigation.

In any case, ideological facts (like the facts of culture in general) must be considered in their context if distortion and confusion of thought is not to result. I hope in a future publication to demonstrate that this point of view must always be taken into account in questions of theory. Anthropology has largely passed over this approach through which is to be found, I believe, special enlightenments rather urgently needed in anthropological studies at the present time.

We need to devote considerable attention to the connections between the ideologies of specific individuals and groups of individuals and what Whitehead has called "that background of unconscious presuppositions which control the activities of successive generations." In other words we must relate the theory of particular anthropologists and of anthropological "schools," to more general prevailing modes of thought. If we wish to comprehend fully the significance of a specific theoretical position we must try to discover the interests and motivations behind the different varieties of organized thought and research. For as Kroeber has observed, "corresponding objectives involve corresponding methods."[27] In sum, any realistic interpretation of anthropological theories must take into account two fundamental factors which are often not explicit in the theory as such: (1) the general intellectual climate (to borrow Whitehead's happy phrase) in which the theory has matured, (2) the questions which anthropologists hope to answer by their researches—this is of course in some considerable measure a function of (1).

The rôle of ideological complexes in the formation of anthropological theories seems to me to have been (to varying degrees) neglected in almost all of the discussions of anthropological theory which have been published thus far. An anthropological approach, however, to anthropological theories would seem to consist precisely in the attempt to see anthropological theories

[27] A. L. Kroeber, *History and Science in Anthropology* (*American Anthropologist*, vol. 37, pp. 539–570), p. 547.

in the broad framework of cultural perspective. Probably the most meaningful contribution which anthropological studies have made to general knowledge is the concept of cultural relativity. But I do not think that anthropologists have adequately applied this insight to their own theories. It would seem that anthropologists have been insufficiently aware of the dominance which has been exercised over its theoretical conceptions by certain sets of postulates which seldom enter into the explicit discussion of anthropological theory.

In short, not only have anthropologists by their too great intellectual celibacy tended to develop what John Dewey has called an "occupational psychosis"; they have also overlooked, in the main, the rôle of "cultural compulsives"[28] in determining the theoretical concepts and postulates which tend to be more or less unquestioningly accepted by most anthropologists in a particular culture at a particular period. It can easily be shown that most theories are intimately related both to the purely personal experiences and "personalities" of their devisers and also to the prevailing patterns of thought. Such a relation, to be sure, does not always or necessarily vitiate any utility the theory may have. But such a view does help us to view theories relativistically rather than absolutistically. Such an awareness contributes toward cleansing our minds from dogma. Sets of postulates we must have, and choice is involved, but ideally dogma has no place in science. Dogma fetters our imagination, dulls our capacity to wonder, so that we stop short of gaining new and fresh insights into cultural reality.

While in certain aspects of scientific investigation it is absolutely necessary that we should take certain things for granted, it is equally necessary that at other times we should consider our subject, coming as close as we can to taking nothing for granted. We must be eternally on guard against the insidious crystallization of dogma (unrealized as such) at the expense of that freshness of outlook which is surely a prerequisite to real scientific discovery. As Bloomfield (and many others) have pointed out, "the Greeks had the gift of wondering at things that other people take

[28] See V. F. Calverton, *Modern Anthropology and the Theory of Cultural Compulsives* (In: *The Making of Man*, V. F. Calverton ed., New York, 1931, pp. 1–41).

for granted."[29] It is that "flourishing freshness" of which Plutarch writes which is, I feel sure, responsible for the fact (or it seems to me a fact, at any rate) that the whole intellectual structure of western European thought has been to a very considerable degree only a parasitic effloresence on the ideas of the Greeks.

I am far from suggesting that anthropologists will attain a Greek intellectual robustness simply by striving for greater intellectual spaciousness and by considering theory explicitly as such. But I do suggest that more sustained and conscious analysis of the more abstract phases of anthropological procedure will have some effect of liberating anthropological intellects. As Mayer has pointed out,[30] all scientific method inevitably has a dual character. There is, on the one hand, verified empirical observation; on the other hand, theoretical analysis. Science is as much a matter of the exposition of relations as of the description of things. Things are the objects of immediate sense perception, but relations are only indirectly—by means of reasoning—reducible to the sense world. Clarification and enlargement of this reasoning element is an urgent need of anthropology.

Concepts to be meaningful must undoubtedly be referable to experience, but this does not unavoidably involve acceptance of the doctrine of radical empiricism that the entities into which scientific analysis involves phenomena must be directly and concretely (rather than indirectly and inferentially) observable. We ought carefully to distinguish the concrete from the empirical (which has a wider sense). Moreover, to repeat once more: any adequate methodology must take account of both the rational and the empirical elements in scientific procedure.

The position of the radical empiricists that theory is simply an epiphenomenon, a reflection, a judgment of opinion which does not have scientific status appears to be a striking example of a too prevalent disposition to dispose of problems by assuming that they do not exist. It is true that many bothersome questions would be eliminated if we accepted this view, and scientific procedure would attain much more easily an intellectually and

[29] Bloomfield (*op. cit.*), p. 2.

[30] J. Mayer, *The Techniques, Basic Concepts, and Preconceptions of Science and Their Relations to Social Study* (Philosophy of Science, vol. 2, pp. 431–484).

emotionally satisfying consistency. But it does not seem to me that any but a moribund scientific conscience would permit such an ostrich-like burying of the head in the sand. What we know at present about the process of making judgments (including scientific judgments) demands that we face the fact (and it is a fact in the sense of being a bit of experience which recurs to different individuals under comparable situations) that the part of theory[31] in arriving at scientific knowledge is an active and essential one which urgently requires systematic and critical examination. Even the most cultivated and sophisticated of minds arrive at assent and dissent rather blindly still. We "know" that we are convinced by an argument but seldom are we able to cite all the steps which from a dispassionate point of view would seem relevant. If scientific work is to attain more nearly to objectivity, the various aspects of this process of acceptance and rejection must be made more fully explicit and conscious.

Otherwise the house that anthropology builds is bound, I think, to fall in tumbling ruins which will not lend themselves to repair or rebuilding. For, howsoever substantial be the bricks by themselves, unless the trusses of the theoretical structure which supports them are sound, the bricks will fall to the ground in a confused mass. A scientific structure, like any other structure, will be stable in so far as not only the primary elements of construction (the building blocks) but also the structural plan which unites and binds together the primary elements and the foundation upon which the whole rests are rigorously tested and examined.

Harvard University, Cambridge, Mass.

[31] I have up to this point deliberately abstained from giving an extended dictionary type definition of "theory." By using the word, by pointing to various relationships I have tried to establish the appropriate "context of situation." For a dictionary type of definition I would suggest approximately the following: "Theory" refers to a statement or statements of somewhat abstract nature covering the relationships between a number of discrete facts. The differentia of "theory" is primarily that the validity of operations of reasoning is at stake as well as the correctness of operations of perception. Theory is dependent upon the logic of inference. Theories depend upon inferences from observation, but cannot themselves be observed directly.

Research Procedure and Laws of Culture[1]

BY

ALEXANDER LESSER

I

WHEN discussions of scientific method shift from natural sciences to social sciences, we tend to feel that we are turning from strict sciences to subject matters which may have achieved some measure of intellectual competence but which lack the rigor, objectivity and principles of organization found in mature science. This sense of a difference between the natural sciences and the social sciences is connected with questions of the meaning and rôle of laws. To the extent to which social and historical events have not been reduced to laws or perhaps cannot be, the disciplines which study such events are considered immature or imperfect sciences. To the extent to which social sciences do not offer predictions, they are felt to have little application to the solution of actual and immediate problems. When a bridge has to be built over a chasm, the laws of the natural sciences can be brought to bear. But when a treaty is to be drawn, where, it is asked, are the social and historical laws which can be applied to the situation?

[1] Read at Second Conference on Methods in Philosophy and the Sciences, New School for Social Research, New York City, November, 1937; and at annual meetings of the Central Section, American Anthropological Association, Milwaukee, Wisconsin, April, 1938.

Laws as statements of some observed regularity in conditions are based upon experience. The experience of order and consistency of some form is prior to the affirmation of the existence of law, and laws when made explicit are formulations of regularities which have been experienced. In all types of experience we base present action upon past experience, constantly anticipating and frequently finding our expectations fulfilled. This is as true in the analysis of social and historical events, and in ordinary social experience, as it is in the physical and chemical dimension. Hence we cannot deny that on the plane of the social and historical we experience order and consistency without affirming as a principle that social and historical events are completely disordered, inconsistent and chaotic. *Some* order and consistency *is* experienced. Therefore, to deny the existence of social and historical laws cannot mean that no regularities are found in such phenomena. Nor, furthermore, can such denial mean that regularities exist but cannot be discovered, since we accept ordered events as existent only because we have experienced such phenomena. Obviously we should be able to find some mode of statement for regularities which we experience. The denial of laws in social and historical events, then, can mean only that we have not as yet succeeded in adequately defining orders and regularities, and that this seems a difficult task.

It cannot be denied that social sciences have thus far failed to formulate statements of order and regularity comparable to the laws of the natural sciences. Where claims for laws have been made they have not received the universality of acceptance from other workers in the same field which is one of the criteria of validity. But so far as social sciences have failed, their failure cannot be credited to the absence of phenomenal regularities on the social plane and must therefore involve shortcomings in methodology and technique.

Traditional concepts of the nature of laws may be involved in the difficulties of the social scientist. Many think of laws as statements of underlying processes which determine cases observed. Something of the older idea that there may be moving purposes in events is involved. When attempts are made to

define laws, the procedure usually moves on the plane of discovering general and all-embracing principles of conditions or developments. Attention tends to be turned away from actual phenomena studied and the obvious and thoroughly accepted beliefs which may be held about it, and toward some special realm of hidden linkages in which laws may be expected to lie. When hidden connections prove hard to discover, there is a tendency to profess a disbelief in laws. Nevertheless the actual procedures of investigators belie such professed statements. The scientist must in practice approach the materials of any given case or problem with certain expectations based upon his past experience and analyses. These expectations or anticipations are not only involved in the terms and concepts used, but include also ways of attacking the problem and a whole body of already accepted ideas about the nature of such phenomenal conditions.

This general fact about empirical procedure is no less true in the social sciences than in the natural or physical sciences. It is as apparent in the work of the social anthropologist studying culture and society as in any other field of social investigation. Unaware of or oblivious to the body of accepted beliefs with which he works as the anthropologist may be, general truths about culture and society can be illustrated from every phase of the anthropological field by drawing upon the fund of 'normal expectations' of the anthropologist at work. In offering a sample selection of such expectations, I am intentionally using the obvious, so that illustrations presented can hardly be called in question if their terms are understood.

2

Under aboriginal conditions of life the only modes of communication are those by personal contact. We observe that wherever peoples are in contact, they borrow from or imitate one another's customs and institutions. Hence a major anticipation in research is that unless recent movements of peoples have occurred in an area, the culture of any group will resemble that of its neighbors. A greater degree of resemblance is expected the

longer the time period of continuous and undisturbed contact. At the same time, in historical analysis we find that the origins of every culture or civilization have been diverse. In research practice these expectations mean that we prepare for the study of a particular group, who are as yet unknown, by finding out all we can about its neighbors and the general area in which it is located. When we study the particular tribe, we look for special historical conditions to explain marked deviations from the norms of culture for the area.

In any cultural group, because of the way in which the individual is conditioned by the behavior of others among whom he grows up, we expect the behavior of the individual to conform to the behavior patterns of his group, or, in somewhat different phrasing, we expect the behavior of the individual to fall within the range of behavior patterns of the group. Furthermore, in terms of behavior patterns, an individual selected at random is expected to prove a more complete microcosm of his group, the more homogeneous the culture. In field practice this means that we rely on evidence based on a limited number of informants or cases the more, the more homogeneous the culture.

Among institutional forms, the universality of a sex division of labor and of an incest rule are suggestive. In any society adult economic occupations or activities are differentiated in some way according to sex. Again, in any society a differentiation occurs between members of the opposite sex with whom sexual relations and/or marriage are socially acceptable and those with whom they are not.

In many phases of culture, as for example in art, we expect certain types of consistency. The recognition of style in the art products of a given people means that if we have examined a limited number of specimens we expect unexamined specimens to conform in many predictable ways to consistencies of style and content discovered in the samples studied. If in a dozen baskets or pots we find the use of several geometrical motives, we expect further specimens to show other geometrical motives and perhaps repetition of some already noted. If another specimen were to differ both in style of arrangement of elements, and content as

regards motives used, we would question whether it actually had been made by artists of the same cultural group. Illustrations of similar types of consistency could I think equally well be drawn from literary forms, ritual patterns, music, kinship modes of behavior and other phases.

Under aboriginal conditions, in many cases little or no trade in essentials is carried on, and there are many situations in which transportation is at a minimum. The environment in such cases affects culture directly. We may say that: The resources of the local environment of any cultural group constitute limitations of the technology and material culture in inverse ratio to the degree to which trade and transportation are developed. In many primitive cultures, in the absence of techniques of food storage and preservation, the food quest is necessarily continuous, and no prolonged periods of leisure can be expected. We study social and political institutions under such conditions in terms of the limitations set by a continuous food quest and the absence of leisure. There are also expectations regarding political institutions under certain conditions. Thus, for example, Boas suggests: "In a region of ample food supply in which the maintenance of life depends solely upon physical skill and in which a sparse population allows everyone to find a productive hunting ground, a differentiation according to rank or wealth is not likely to occur, except in so far as orphans, widows and old people may be thrown upon the mercy of their friends and relatives."[2]

The world-wide system of production and exchange which we know today is a recent phenomenon. Among aboriginal peoples, in only a few regions are cultural groups dependent parts of a large, intertribal system of exchange. The more general tendency is toward relatively independent, self-sufficient groups. Where that is the condition, the density of population is linked to the form and character of the economy, a fact emphasized by many writers. Boas discusses this in the following: "We may recognize definite, causally determined relations between the economic conditions of a people and the size of the population.

[2] Article, *Anthropology*, Encyclopedia of Social Sciences, p. 100.

The number of individuals of a hunting tribe inhabiting a particular territory is obviously limited by the available amount of game. There will be starvation as soon as the population exceeds the maximum that may be maintained in an unfavorable year. If the same people develop agriculture and the art of preserving a food supply for a long period, a denser population is possible, and, at the same time, each individual will have more leisure and there will be a greater number of individuals enjoying leisure. Under these conditions the population is liable to increase. We may perhaps say that complexity of culture and absolute number of individuals constituting a population are correlated. Whether this development actually occurs in a given population is an entirely different question."[3] In other words, complexity of culture cannot be expected unless some degree of population density is found, but populations may conceivably be large and culture relatively simple.

Expectations as regards the forms of property and ownership can be illustrated. Whenever complete objects are manufactured by individuals, if the individual maker is not a slave, has not been hired, and has not been delegated, the objects he makes are his for use and disposition, and such individual ownership is socially sanctioned. As regards my own experience of the data, I would affirm that in a region of uniform economic conditions involving hunting and food-gathering peoples who lack arts of storage and preservation, raiding for property does not occur. Boas notes that "whenever the transfer of property involves future obligations on the part of the recipient, the transaction is public and gives opportunity to the development of ritualistic behavior."[4]

Many illustrations of expected relations could probably be drawn from the more technical field of primitive social organization. In fact it is in respect to this aspect of culture, particularly as regards forms of marriage and social organization, that there is a long history of attempts from Morgan's day to the present to define consistent interrelations or laws. Rivers, for example,

[3] Anthropology and Modern Life, pp. 213 ff.

[4] Article, *Anthropology*, Encyclopedia of Social Sciences, p. 100.

asserted in a classic passage in relation to the "determination of the nomenclature of relationship by social conditions," that "we have here a case in which the principle of determinism applies with a rigour and definiteness equal to that of any of the exact sciences."[5] As an illustrative suggestion in this field I would state that where prescribed primary marriages are the social rule, affinal kinship terms and usages will reveal the character of the prescribed marriage. Native Australia is of course a main case in point, where rules prescribe the marriage of certain kinds of cousins. As a result, relatives by marriage are also relatives by consanguinity, and terms and usages for relationships reveal the facts. In practice the principle I have mentioned means that if a field worker were to report the existence of a rule of marriage among a certain tribe, yet could not describe the form of the rule, we could infer the nature of the rule from the kinship terms and usages for relatives by marriage. If no consistent marriage pattern were implied by them, we would doubt the existence of any prescribed rule of primary marriage.

Boas indicates a specific expectation as regards marriage forms: "If polygamy is combined with permanent matrilocal residence, the wives must necessarily be sisters."[6]

The field of religion could be drawn upon also for illustration. For example, a theological systematization of religious thoughts and concepts cannot be expected in the absence of a priesthood. Or, quoting Boas on the reinterpretation of traditional behavior and belief, "It may be claimed as a general rule that interpretations of customs and attitudes do not agree with their historical origin but are based on the general cultural tendencies of the time when the interpretation is given."[7] The general experience of investigators of the functional rôle of ceremonial and ritual forms has been commented on by various writers. Boas summarizes the basic situation by saying: "Important actions, when accompanied by difficulty of execution and likelihood of failure, or those involving danger, give rise to a variety of types of cere-

[5] W. H. R. Rivers, Kinship and Social Organization, p. 93.
[6] Article, *Anthropology*, Encyclopedia of Social Sciences, p. 87.
[7] Anthropology and Modern Life, pp. 213 ff.

monial behavior. The making of a canoe is often an act of great ceremonial importance, as in Polynesia, or is accompanied by superstitious beliefs and practices, as on the Northwest Coast of America. Hunting and fishing on which the sustenance of the people depends, agricultural pursuits, herding and war expeditions, are almost always connected with more or less elaborate ceremonials and complex beliefs, on the whole the more so the more deeply success or failure affect the life of the people. We may recognize an expression of the same 'law' in the formal celebrations with which we like to accompany the achievement of great technical undertakings, the completion of the education of the young, or the opening of an important assembly."[8]

Perhaps most controversy has surrounded questions of laws of development and of predictions about the future. Here too, however, one can illustrate how experienced consistencies have affected our expectations. While we do not accept the exact sequences postulated in early schemes of the evolution of economic forms, we can nevertheless assert that in historical development the presence of certain forms necessarily implies the earlier presence of certain others. For example, any culture with agricultural or pastoral techniques must have been in a condition without them at some time of its historical past, viz., in some condition of hunting, fishing, food-gathering economy in which resources of nature were garnered as found. Cultures which are complex, with specialized institutions and an intensive division of labor, necessarily imply a historical past of simpler institutional forms and less specialization.

The possibility of prediction about the future is often denied. Yet consider cases such as the following: We know from repeated instances that wherever in island regions of Melanesia certain forms of imperialistic exploitation have been practiced on native groups, certain types of economic and cultural breakdown have occurred. The same methods of exploitation are now being carried out in New Guinea that were used many years ago in other Melanesian areas. In interior New Guinea there are still

[8] Idem., p. 214.

peoples who have never been seen by Europeans. We can predict that, when these groups are discovered, if they are exploited in the same imperialistic modes which were used elsewhere in the region, certain definite conditions of cultural breakdown will rapidly follow.

3

These somewhat random examples illustrate the character of normal expectations that are basic in contemporary anthropological research. If anthropologists were in the habit of making expectations more explicit, I believe that illustrations could be multiplied indefinitely.

The rôle of such expectations in inquiry and research is in the first place similar to the rôle of expectations in ordinary experience. Anticipations based on past experience are used, consciously or unconsciously, in dealing with present or impending situations. But further, such expectations are of the nature of hypotheses, linking former investigations and research to the case in hand. Whether explicitly stated, or implicitly felt, they affect inquiry and treatment, determining to a large extent what we take for granted and where we give special attention to unusual phenomena. These expectations of the investigator reflect the consistencies and types of order or invariant relations that have been so far experienced by him. If framed as hypotheses, in such form that they can be subjected to verification in any given case, they become essentially the laws with which the investigator is actually operating in his research.

In the study of peoples and cultures, these normal expectations of the investigator are usually the last things to be made overt. They are most of the time somewhat unconscious. Attention is directed not at them but at something else, viz., the particular and special conditions of the particular and specific case. Thus arises the tendency in modern anthropology toward a cultural atomism which seeks to describe the particularity and uniqueness of the special culture without realizing that its very difference from other conditions can be rendered adequately only against the background of its similarities to them. In spite of its unique-

ness it is a society and a culture and there are many features implied by that fact alone, which are too often disregarded as over-obvious. Hence, instead of the particular case serving as an experiment in terms of which expectations can be consciously verified or disproved, it is too apt to be taken as a case which disproves the existence of general homogeneities.

If we turn for laws to the expectations of the scientist at work, the instrumental character of such laws is immediately apparent. Expectations cannot be fixed or final statements of immutable and eternal qualities or relations, but in the form of laws are clearly tools forged by the investigator in his experience and work by means of which he seeks to bring to bear the fruit of his earlier experience in the analysis and control of the present case. In so doing the present case takes on new meaning. In relation to laws as hypotheses or working expectations, the case which is studied at any time becomes the experiment by means of which the hypotheses are verified or disproved. Hence every specific and controlled research performance, if its relation to former cases is exactly charted in terms of hypotheses, constitutes a verification of the whole system or set of hypotheses and laws already realized. At the same time, it offers, because of its particularity, novel conditions which may generate additional expectations or hypotheses. The constant emphasis in social sciences upon research is then seen in its true light. It serves the function of experiment in other sciences, viz., that hypotheses or laws are being checked and verified by the conditions in a specific case.

The fruitfulness of this concept of the nature of laws for anthropology seems to me great. Modern ethnologists tend to emphasize the need for more intensive analyses and studies of specific cultures or areas. But in carrying out such research the usual procedure is to look to each culture to suggest novel and special problems, often tangential to earlier and other work. If expectations are made overt as hypotheses, this turning to intensive analysis of a particular culture has the genuine significance of an experiment in culture, and the special or particular conditions encountered will not be lost sight of, but take on new meaning.

One of the main objections to attempts to frame laws of social and historical phenomena has been that they can have little applicability to actual particular situations. It is claimed that in isolating what is common to many instances, we must necessarily arrive at abstract generalizations which can have little relevance to action in a particular situation, with all its local uniqueness. But if, instead of using methods of progressive abstraction, we approach the problem of laws in terms of normal expectations employed in dealing with a particular case, and subject to verification and disproof in terms of that case, the applicability of the revised expectations to the next particular case is enhanced. Laws as instrumental hypotheses linking past to present experience will have the more applicability to particular situations the more they are used explicitly in action and revised to meet the needs of actions and situations.

Constant practice of such a procedure seems bound to sharpen our awareness of the aspects of our own social experience which can be framed as hypotheses and subjected to verification or disproof. The keener our awareness becomes the more the social scene is likely to reveal of homogeneity, order and consistency in experience, which can be used in the control of social events and situations. Laws for social action must be derived from actual experience and practice in dealing with social events and phenomena and only then can be expected to have fruitful application to the problems of social life. To derive such laws a fundamental requirement is that we make more explicit the assumptions or expectations which link past experiences to present situations, within any special or technical discipline, and ultimately within the milieu of social experience itself.

Columbia University, New York.

Natural Languages as Cultural Indices

BY

W. H. WERKMEISTER

A SHORT time ago, D. D. Lee wrote in *Philosophy of Science:* "Grammar contains in crystallized form the accumulated and accumulating experience, the Weltanschauung of a people."[1] He thus called our attention once more to a theme which was much discussed during the 19th century but which has been in disrepute for some time in philosophical circles. It is a theme, however, which is not without merit. More than seven years of intensive study of primitive languages have convinced me that the work started by Wilhelm von Humboldt deserves serious consideration even at present, and from the ethnological as well as from the philosophical point of view.

I realize, of course, that a study of this kind is not a simple matter. In the first place, no individual investigator can become acquainted with all languages in their native environment. We must necessarily rely upon secondary sources, and these are not always trustworthy. The grammars and linguistic summaries at our disposal are often inexpertly written or compiled and are therefore faulty and misleading. Nor do they always contain the information we need.

As the science of linguistics advances, special investigators will

[1] Lee, D. D., Conceptual Implications of an Indian Language, *Philosophy of Science*, V (1938), 89.

undoubtedly augment whatever information we now possess, and will place at our disposal their expert knowledge of individual languages. But even so, certain difficulties of interpretation must remain; for languages can be fully understood only through their use in the original cultural settings in which they were developed, and no one person can ever master all languages in this sense. Book-learning alone, however, can not give us the understanding which comes from living contact with a culture and its language.

In addition to these practical difficulties there remains one of principle also. Wilhelm von Humboldt conceived and developed with great sagacity a "general theory of language",[2] according to which all languages were to be arranged in a single line of development from the most primitive form to the most advanced structure; and the different cultures were assumed to correspond to this simple arrangement. As a matter of fact, empirical research contradicts all preconceived notions of this type. Where Humboldt postulated uniformity of development (as the outcome of the unfolding of an "inner language form"), we are to-day confronted with the greatest imaginable variety of forms and functions, and with divergent lines of progression, and our subject matter has become so complex and entangled that we no longer know just what bearing the study of any one language may have upon other linguistic forms and structures and upon our problem as a whole. Abstract speculations do not help us. We must deal with our problem empirically and must study the languages and cultures as we find them. Whether such 'empiricism' has any philosophical significance or not remains yet to be seen.

Wundt, as may be remembered, spoke of "subjective" and "objective" languages,[3] which he regarded as appropriate expressions of corresponding mental attitudes and in terms of which he interpreted various culture types. The contrast of Chinese and Arabic language forms illustrates clearly his point of view.

[2] Humboldt, W. v., *Ueber die Verschiedenheit des menschlichen Sprachbaues*, Pott edition, II. For a criticism of this thesis, see Steinthal, H., *Die Classification der Sprachen dargestellt als die Entwicklung der Sprachidee.*

[3] Wundt, W., *Voelkerpsychologie*, I, 451–52.

In Chinese, for example, complex actions are broken up into their component parts or phases. One does not 'fetch' me something, but one 'goes takes comes gives me'. The individual words of the language undergo no change; and the interrogative (in as far as it is used at all)[4] is placed at the end of the question, which in every other respect is indistinguishable from an assertion. Then, too, time and space designations as well as the subject (and at times even the object) precede the verb.[5] It is as if a stage were being built upon which the action is to take place; then the actors are introduced and, finally, the action follows. All this is characteristic of languages which Wundt regarded as "objective", i.e., of languages which seem to depict calmly all things and events in their space-time settings and as they present themselves to a dispassionate observer.

In Arabic, on the other hand, the verb usually stands at the beginning of the sentence: *wayanqasimu 'lquṭru 'lmiṣriy ilā qismaini*, and is divided the land the Egyptian into two parts.[6] In this way we are being confronted at once with the 'action' instead of with the 'stage settings'. The interrogation always introduces the question: *a' qatala 'lisanu 'lmolaka*, has killed the man the king?; thus presenting again all emotionally accentuated parts of the sentence at its very beginning. In addition, the intensification of the emotions often finds an immediate linguistic expression in the reduplication of the second consonant: *qatala*, to kill; *qattala*, to murder; while the mere intention to do something may be indicated by a lengthening of the first vowel: *qâtala*, to want to kill. All these linguistic characteristics typify the 'emotionally agitated', and in this sense "subjective", languages of Wundt.

More recently, Graebner has called attention to the close relation which exists between the shamanistic world-view of 'arctic cultures' and the language of the Eskimo.[7] The poly-syntheti-

[4] Often the Question is expressed by an assertion and its contradiction: *na ko jên lai pu lai*, that man comes not comes.

[5] Summers, J., *Handbook of the Chinese Language*; Lessing and Othmer, *Lehrbuch der nordchinesischen Umgangssprache.*

[6] Thatcher, G. W., *Arabic Grammar*, 2nd edition; Socin, A., *Arabic Grammar*, 2nd English edition.

[7] *Cf.* Graebner, F., *Das Weltbild der Primitiven*, 93–103.

cal changes of words seem to reflect something of the shamanistic belief in the animistic transformations of a reality which is 'alive' in all its manifestations. Even the bone-knife in the hand of an Eskimo may take on human form! Linguistically we obtain such compounds as the following: *aulisa-ut*, fishing tool; *aulisa-ut-isak*, fishing tool usable as (*i. e.*, something that may be used as a fishing tool); *aulisa-ut-isar-siwunga*, fishing tool usable as obtaining my; *aulisa-ut-isar-si-niarpunga*, fishing tool usable as to obtain looking my (*i. e.*, I am looking for something that may be used as a fishing tool). The progressive modification of the additional suffixes indicates that the whole sentence may be regarded as a single word.[8] And "the words of the Eskimo are born on the tongue on the spur of the moment"; "new combinations are specially formed to meet the claim of every situation". As a result, as far as word-formations are concerned, the Eskimo language is "incessantly *in statu nascendi*",[9] constantly undergoing modifications and transformations for which only the shamanistic world-view can find a counter-part in the external world. Nor is this all!

Surrounded by a barren nature which forces a severe struggle for existence upon him, and hard pressed by the 'evil spirits', creations of his own imagination, the Eskimo has developed a world-view which is far from reflecting the exhuberant buoyancy of conquering heros. On the contrary, the whole gloomy desolation of his environment permeates it. And the defensive attitude of the Eskimo seems to find expression even in the structure of his language;[10] for events and actions are described in nominal rather than verbal form, which makes it appear as if the active subject were passively suffering the action. Thus, the Eskimo does not say, 'I am happy', but *kuviasukpunga*, happy being my; not, 'he rides the kajak', but *kajaktorpok*, kajak riding his; not,

[8] *Cf.* Stefánsson, V., *My Life with the Eskimo*, 356. Thalbitzer, W., Eskimo, *Boas' Handbook of American Indian Languages*, I, 1002, 1053: "A whole sentence may be expressed in a word—in a word-sentence". Bourquin, Th., *Grammatik der Eskimo-Sprache*, 167.

[9] Thalbitzer, *op. cit.* 1055–56.

[10] *Cf.* Finck, F. N., *Die Sprachstaemme des Erdkreises*, and *Die Haupttypen des Sprachaues*, 35–36.

'there began to be men', but *inu-ka-li-lauk-pok*, men to have began it.[11]

In view of such facts, Thalbitzer, too, comes to the conclusion that "it can not well be denied that in the signification and use of the forms certain logical and fundamental differences from the grammatical system of our language occur, which differences give evidence of marked peculiarities in the psychic basis of the Eskimo language."[12] And Stefánsson maintains that "one has really to learn a different method of thought before one can acquire versatility in expression" in Eskimo.[13]

Of course, I do not maintain that the correlations of language and culture suggested by Graebner and Wundt have been proven beyond doubt. Nor do I disregard the fact that Wundt's classification of languages is impaired by extra-linguistic considerations which are rooted in the psychological preconceptions of its author. Nevertheless, there can be but little doubt that the difference between the older and the more recent linguistic forms corresponds to a difference in the ways of thinking. Primitive languages are notoriously concrete in their expressions, designating, for example, specific animals and plants in great variety and with minute shadings of meaning, but lacking words for such simple abstractions as 'animal' or 'plant' in general. And conjunctions of a purely formal or logical significance, such as our copula, are rare or entirely wanting. They are obviously a very late product of linguistic development and appear only when thought has reached a high level of abstraction.

All 'natural' languages develop out of, and in conformity with, *usage*, *i.e.*, they develop in inseparable conjunction with activistic situations and with specific tribal reactions to environmental conditions. And they bear the stamp of these situations and reactions.[14]

The comparison of languages shows beyond any possibility

[11] Compare these linguistic forms with the English phrase, *thoughts press in upon us.*

[12] Thalbitzer, *op. cit.* 1057.

[13] Stefánsson, *op. cit.* 354.

[14] *Cf.* Werkmeister, W. H., The Symbolism of Natural Languages, to be published shortly as Chapter V in the author's *Philosophy of Science.*

of doubt that, "in various groups of mankind, the mind responds quite differently when exposed to the same conditions;"[15] and nowhere is a linguistic expression a mere copy of an object. Every symbol of every natural language is but a vehicle or link in a process of interpretation in which the experiencing subject 'comes to terms' with its world. In this process, subjective factors, abilities and interests, are inseparably intertwined with objective conditions, and 'things' and 'actions' become meaningful only relative to certain anthropocentric 'perspectives'.[16] The manifold complexities of sensuous impressions are molded and shaped by interpretative thought and linguistic means into 'things' and 'actions', 'states' and 'qualities'.[17] To the extent to which this is true, it must be admitted that many different solutions are possible; and the variety of existing languages confirms this deduction.[18]

Once the linguistic forms and syntactical structures have been established, they in turn begin to influence the thoughts and ideas expressed by means of them. While language presupposes thought, linguistic habits color our thinking; and it is well to realize that "we accept facts as language presents them to us", that "we think of verbal images in the form in which language clothes them."[19] "The powerful reflex influence of language on mental action is a universally admitted fact in linguistics; to allow it is only to allow that rooted habits, learned by each generation from its predecessor, have a controlling influence on action—which is axiomatic."[20]

I do not contend, of course, that linguistic forms are an absolute barrier that would prevent a people from developing more generalized or new forms of thought; but even Humboldt did not

[15] Boas, F., *The Mind of Primitive Man*, 98. For detailed proof see Werkmeister, *op. cit.*

[16] Werkmeister, *op. cit.*

[17] *Cf.* Cassirer, E., *Philosophie der symbolischen Formen*, I, 231–32. Humboldt, *op. cit.* 72–73.

[18] *Cf.* Werkmeister, *op. cit.*

[19] Vendryes, J., *Language, A Linguistic Introduction to History*, English edition, 73. See also Gardiner, A. H., *The Theory of Speech and Language*, 327.

[20] Whitney, W. D., *The Life and Growth of Language*, 225.

maintain this. He admitted that the "creative power" of mind is capable of modifying linguistic forms and structures.[21] This does not warrant the conclusion, however, that therefore differences of languages do not indicate fundamental differences in attitudes and world-views. Even so cautious and critical an investigator as Vendryes is forced to concede that there exists a relation between mentality and language, and that "sometimes language can even modify and regulate mentality."[22] More than this, he maintains that "we know very well that a language is the reflection of human consciousness", and that it "betrays the nature of the mind which conceives it."[23]

But if this is so, then there is no reason why languages should not be useful as cultural indices—provided a definite correlation between language types and culture types can be established. Humboldt assumed such a correlation; Wundt attempted to demonstrate it in his *Voelkerpsychologie*, and even Vendryes who, according to the editor of the series in which his book appeared, "justly criticizes the introduction into linguistics of the concept of race" and "also objects to the notion of an ethnic mentality", "was obliged to recognize that there is some relation between the mentality of a people and their language."[24] The question is, What is this relation and how can it be established?

The attempt has been made to classify languages in accordance with the way in which they express 'action';[25] and it must be admitted that such classification is at least possible. Languages often are either predominantly 'nominal' or predominantly 'verbal' in character. If the nominal type is developed in all strictness, the structure of the language is determined and governed by the notion of the 'object'. In such cases, "the verbal idea has not emancipated itself from the idea of things that may

[21] Humboldt, *op. cit.*, 204–215.

[22] Vendryes, *op. cit.* 239: "The habit of always placing the verb in a certain position leads to a particular manner of thinking, and may have a certain influence on the course of an argument. French, German, or English thought is subordinate to a certain extent to the language."

[23] *Ibid.* 357.

[24] *Ibid.* xiii. See also 354–59.

[25] *Cf.* Finck, *Hauptypen*, *op. cit.* 13–14.

be owned, or which are substantial."[26] The Eskimo, for example, "does not say *he stabs me, he sees me*, but rather *my being stabbed by him is, my being seen by him is.*"

On the other hand, in all verbal languages grammatical forms and syntactical structures are dominated by the verb; and this may go so far that even adverbs of location really express or refer to motion. Position is then defined as a result of motion, as, for example, in Tsimshian, which requires that we say *he is placed toward a place near by* rather than simply *he stands near by.*[27]

The languages of the verbal type may then be subdivided into two groups, depending on whether the verbal expressions interpret motion predominantly as an objectively occurring and objectively observed process; or whether they emphasize the activistic part played by the subject, its doings and exertions. And so we obtain three language types, which Cassirer associates with specific cultural centers,[28] such as the Altaic region, and continental North America. The classification itself, however, encounters serious difficulties. Its linguistic criteria are not as definite and decisive as we might wish them to be, nor do they affect the most important aspects of language. In addition, the correlation of these language types with specific culture types is far from being satisfactory. Inconsistencies and overlappings abound. We shall therefore adopt a different basis for classification and shall accept the coincident distribution of language types and culture types as proof of the correlation.

Our principle of classification for the natural languages is to be free from all extra-linguistic considerations, and I believe that the distinction between 'prefix languages' and 'suffix languages' will serve our purpose well. For the structural differences of these language types are undeniable and are often far-reaching. Prefix languages are usually also characterized by a post-position of the possessive genitive, while suffix languages prefer its pre-

[26] Thalbitzer, *op. cit.* 1059. Thalbitzer discusses here the Eskimo language only, but his statement is true universally.

[27] Boas, F., Tsimshian, *Handbook, op. cit.* 299.

[28] Cassirer, *op. cit.* 235–43.

position. Since the position of the accusative as well as that of the adjective follows, as a rule, that of the genitive, the 'prefix-suffix' criterion really designates two fundamental language types. And from the ethnological point of view it is significant that these language types are closely affiliated with distinct culture types.[29]

If we add to our criterion linguistic considerations pertaining to word-endings, to principles of classification, and to the use of various number systems, we find that the regional distribution of distinct language types corresponds definitely to the regional distribution of equally distinct culture types. An examination of more than 2,500 languages and dialects shows that, on the whole, the following correlations prevail:

	MOTHER-RIGHT CULTURES	FATHER-RIGHT TOTEMISTIC HUNTERS	FATHER-RIGHT NOMADIC HERDSMEN
1. Position of Genitive:	Complete post-position (this is true especially in the oldest cultures; it is not quite so definite in America)	Predominant pre-position	Complete pre-position
2. Word-Endings:	One consonant	Vowel endings	Many consonants (especially s-combinations)
3. Classification:	In southern regions masculine-feminine; in northern regions animate-inanimate	Person-thing	
4. Number Systems:	Quinar-decimal	Quinar-vigesimal (body cultures)	Pure decimal

This correlation of the regional distribution of the language types with that of the culture types is "nearly perfect" as far as the most primitive cultures are concerned.[30] In the case of higher

[29] An abundance of relevant material and the most comprehensive bibliography in the field may be found in Schmidt, W., *Sprachfamilien und Sprachenkreise der Erde*, especially 271–540.

[30] *Cf. ibid.* 529.

cultures some fluctuations occur. But nowhere do we find the language types scattered about in disconnected little territories. On the contrary, the evidence clearly indicates the existence of certain central regions of distribution which cover large areas, and from which structural influences radiate in ever widening zones of transition. This corresponds exactly to the distribution of culture types and their inter-mixture.

We find, for example, that the Ural-Altaic languages, *i.e.*, the languages of the distinctly father-right cultures of the nomadic herdsmen of Central Asia, showed originally complete pre-position of the genitive; and this linguistic peculiarity carried over even into the Indo-European languages of all the people forming a secondary belt around the primary cultural center. In the Hamitic languages, too, the pre-position of the genitive is the older form, and its regional distribution corresponds once more to that of the appropriate father-right cultures of nomadic herdsmen.

Mother-right cultures, in old and new forms, abound in Australasia, the Austro-Malayan area, and in Annam. The Australasian languages with their complete post-position of the genitive therefore typify these cultures. Traces of these languages are found in the western Himalayas and, via the Elamitic, far into North Africa. And this vast region, too, is one of distinctly mother-right cultures, or shows at least traces of mother-right. In Africa, finally, we find complete mother-right cultures and their various mixed forms only in the regions where the languages show complete post-position of the genitive. Here the correlation is practically perfect. Even in America the centers of a complete post-position of the genitive seem to be embedded in mother-right territories. This is true in the Northwest, in Central America, and along the coast of South America. But in America, too, we find the greatest and most noteworthy discrepancies in the regional correlation between culture types and language types. It is characteristic for many of these discrepancies that regions which have no mother-right cultures but do have complete post-position of the genitive are wedged in between mother-right cultures or cultures with traces of mother-right. We find this to be true, for instance, in California.

From similar occurrences in Africa and Australia, Schmidt infers that the post-position of the genitive may be originally characteristic of still more primitive cultures which in due time gave way to mother-right cultures.[31] In harmony with this thesis would be the fact that the correlation of the post-position of the genitive and mother-right cultures is most perfect in the older cultures, and that it becomes less definite in the younger forms of mother-right cultures. Further investigation is necessary if we desire a better understanding of these relationships.

It was not my purpose to present as final, as proven in every detail, a thesis which is alluring but beyond the realm of rigoristic proof. However, in view of the evidence at my disposal, I am satisfied that the correlation of language types and culture types, as here suggested, is positive and is sufficiently well established to deserve serious consideration. Natural languages *are* cultural indices. Indeed, it would be strange if they were not; for languages are but integral parts of man's cultures, and both are specific responses to fundamental problems and conditions. That a mentality which creates similar cultures should also create similar languages is not an unnatural contention.

University of Nebraska.

[31] *Ibid.* 461–62.

DISCUSSION (CORRESPONDENCE)

In this section we shall publish brief communications commenting on published papers or bringing up special points of interest which can be discussed by correspondence.

THE REALITY OF THE ATOM

Dear Sir:

In the issue of Nature for July 16, 1938, "A.F." has given us a review of Darrow's "Renaissance of Physics". While expressing approval of the book as a whole, he criticises Darrow as a "pseudo-philosopher" because of his expressed belief in the reality of the atom. This raises the interesting question of why the scientist believes in the atomic nature of matter.

The reviewer's fundamental assumption seems to be that, in the physical world, only the objects of direct sense perception are real in any significant sense. His table is "real" because it can be seen and felt. The meteor is "real" because we can see it. The question of the "reality" of the atom is "irrelevant" because it cannot be seen or felt—irrelevant for this reason, "at least until some miraculous change in the scale and powers of our being may bring the atom into the region of direct sense-perception".

Does this imply that, while this side of the moon is real, the question of the reality of its other side is irrelevant?

The question naturally arises; Does the Reviewer believe that the table in question has an external existence independent of his perception of it, or does he believe that his own perception of it is the whole of the reality? If the latter is true, then he is logically a solipsist, though I suppose that most of us agree with Professor Pratt that there never was an actual specimen of the species Solipsist.

But if he believes that the table has an actual existence, independent of his perception of it, then he must have developed some process of inductive reasoning by which he has acquired this belief in the existence of a real something beyond and independent of his own sense-impressions

—a something which influences his conscious experiences called sense-impressions, but which, in the nature of the case can never be directly experienced by him.[1]

But what is this inductive process, which enables us to gain, from subjective sense-data, a belief in the external table which cannot be directly experienced? Is it not the same *in kind* as that by which we gain a belief in the reality of the meteor which leaves behind it a streak of light in the sky—and a belief in the reality of the atom which gives rise a streak of water droplets in the cloud chamber? Is it not artificial to claim reality for the external object table, which by a series of intermediate physical processes influences our sense-impressions; while claiming irrelevance for the concept of reality as applied to the atom, which by a series of intermediate physical processes influences our sense-impressions? In our search for reality, why should we assume that the atom is unreal while the table is real, simply because in the case of the atom its smaller size necessitates a larger number of intermediate physical processes to enable it to influence our sense-impressions?

It may well be claimed that the evidence for the external reality of the table is greater than that for the external reality of the atom. But does this justify classifying as a pseudo-philosopher one who believes that the evidence which has accumulated for the reality of the atom is exceedingly, almost overwhelmingly, strong?

Elsewhere the Reviewer writes; "It is possible to develop a consistent scheme of physical thought without committing one's self to the tenets of any one of a half-hundred jarring philosophical sects. One does this by refusing to confuse concept with percept; by recognizing that the conceptual model *is* a model, changing with increase of knowledge."

But is it really true that the scientist can dispense with all philosophical foundation for his scientific reasoning? Is it not true that he actually *is* either a solipsist or a believer in external reality?

And just what is this fundamental philosophical distinction between "percept" and "concept"?—the "percept" of the "real" table and of the "real" meteor, and the "concept" of the atom, the very idea of whose reality is irrelevant? Are they not both mental constructs of what we have come to believe exists in the external world? And is not

[1] Some philosophers, of course, hold that we have a *direct knowledge* of the external world, which does not involve any inductive reasoning between our sense-impression and our *knowledge* of the reality of the external object. But various lines of evidence convince most of us that man has no such vast power of *direct knowledge* as this would involve.

our belief that these mental constructs—these models, table, meteor, atom—correspond more or less closely to external realities—is not this belief, in the case of the table as well as the atom, the result of inductive reasoning applied to our subjective sense-impressions? We cannot doubt the reality of our conscious sense-impressions without doubting fundamentally the facts of consciousness, our sole source of data for intellectual activity. But sense-perceptions are something quite different. They are our *beliefs about* external realities. We can and do doubt *them*. We recognize that they are sometimes mistaken conclusions about "external realities" that are not there. Is there any fundamental philosophical distinction between the method of acquiring our "perceptual" model of the table and our "conceptual" model of the atom?

It may be of interest to analyze a little more in detail the case of the meteor and the atom. Those of us who are not solipsists might describe the situation somewhat as follows:

A certain solid body, a meteor, passes through the air, leaving behind it in the sky a streak of incandescent matter; light from this streak enters the eye; the lens focuses it to an inverted image on the retina; nerves carry something to a brain center; a subjective conscious experience follows called a sense-impression; this is followed by a mental construct, a percept, of a streak of light in the sky. The streak of light is not the meteor but light from what the meteor left behind it in its path. We all believe in the reality of the meteor in the sky because, in the light of corroborating evidence, this is the best explanation we can find for the sense-data in question.[2]

An ionized atom passes through the cloud chamber giving rise to a streak of water droplets; light is reflected from the droplets to the eye; a retinal image, nerve action, sense-impression and a percept of a line of water droplets follow. We conclude that there is a real line of water droplets in a real cloud chamber, which we believe to be caused by a real moving atom. We believe this because this is the best explanation we can find for the sense-data involved.

There is other corroborating evidence for the reality of the meteor. We can see solid meteors on the earth, and by a course of inductive

[2] Of course the inductions involved in "seeing" an external object have long since become subconscious and instinctive, like the volitions which guide the fingers of a Paderewski. But this should not obscure the fact that our instinctive association of the sense-impression with the external object was acquired by inference from sense-data, and needs frequently to be checked in order to avoid error.

reasoning we conclude that they fell upon the earth from the sky, leaving a streak of light behind them as they fell through the air and that the reality of such a falling meteor is the best explanation of the sense-data involved in our "seeing" a shooting star.

We also have a large amount of corroborating evidence for the reality of the atom—evidence from chemistry and physics, especially from modern physics with its wave mechanics. This evidence is such as to make it seem to the present-day physicist extremely probable that matter is composed of atoms, though the ultimate nature of the atoms is still very doubtful in several regards.

It is true that we cannot *see* an atom. We can see interference figures caused by X-rays reflected from groups of atoms in such a way as to teach us just how the atoms are arranged about each other in the crystal lattice, at least so Mr. Bragg and the rest of us believe. But this is not seeing an individual atom. We can see minute individual particles, which in some cases are similar to, if not truly, molecules. We can *see* them being bumped about by what we believe, on other evidence, to be actual invisible molecules and atoms. We have excellent evidence that there is every gradation in size and velocity between the visible Brownian particles and the invisible atoms that are kicking our Brownian particles around. Is it clear why we should conclude that the Brownian particles are "real" because light can be reflected from them to the eye; while holding that the idea of "reality" is irrelevant when applied to particles just too small to thus reflect light?

The above, and a large volume of other data, form a great interwoven mass of evidence for the reality of the atomic nature of matter. We could hardly ask for a finer body of verifying data for our hypothesis. This does not *demonstrate* the reality of the atom. Science never *demonstrates*. It does not tell us all we would like to know about the atom. Many *important* points are still in the offing. But it has made the atomic nature of matter seem exceedingly probable to the modern physicist.

The view that seems to me worth emphasizing is that *all* our beliefs in external reality are the result of inference from our subjective sense-data. If this is true, then there seems to be no fundamental philosophical distinction *in quality* between the evidence for the reality of objects which we can "*see*", and for the reality of those which can never be "seen". The difference is between different amounts of evidence which we have for the reality of different external objects, not between fundamentally different kinds of evidence, or different kinds of reality.

There are able philosophers who do not agree with the "causal theory" of perception here presented. But its standing in the world of philosophical thought is such that it does not seem justifiable to classify as pseudo-philosophers all those who believe it to be the soundest theory, in view of all the available data.

WILMOT V. METCALF

College Hill
Clinton, N. Y.

EMPIRICAL INDUCTION BY PROBABILITY AND THE EXTERNAL WORLD

Dear Sir:

Mr. Williams, in his argument for the reality of the external world (*Philosophy of Science*, Apr. 1938, p. 184), is driven to the confession, "The principle of indifference appears to be necessary to secure a strictly prior probability to any hypothesis." It may be argued that such an appeal is admitting the impossibility of empirical induction by probability, for the following reasons:

A. This principle, that "prior to all evidence, any hypothesis has a probability of $\frac{1}{2}$", looks like a way of *drawing knowledge out of ignorance*. We know nothing about a hypothesis, *ipso facto* it has a finite probability. But if we know nothing about it, how can we know anything about it?

B. This principle leads to absurdities. For example, suppose we consider the proposition, "A comunjurum is green." Then, on Mr. William's reasoning, two things follow: (1) The antecedent probability of this proposition is $\frac{1}{2}$. (2) The logical sum of this proposition and its contradictory equals certainty.

Suppose it then appears that "comunjurum is logical equivalent to "square circle." Both propositions (1) and (2) are consequently seen to be false! Then how about the correctness of the principle of indifference? And about Mr. Williams' proof for the external world?

HOMER H. DUBS

Duke University

PROFESSOR DUBS ON THE PRINCIPLE OF INDIFFERENCE

Dear Sir:

In my discussion of Professor Nelson's skepticism toward the argument for an external world, I was less concerned to defend the principle

of indifference than to maintain that if, as Mr. Nelson claimed, the proof of an external world demands an *a priori* probability to start it off, then (1) every empirical proof or confirmation requires the same; and (2) only the principle of indifference, or something very much like it, can furnish such a probability. Beyond this, I only suggested that the principle of indifference is not as black as it has been painted.

Professor Dubs's paradoxes, at the expense of the principle, I do not consider at all fatal. (A) For the first, we may paraphrase Jeffreys (*Scientific Inference*): if we cannot draw knowledge out of ignorance, to begin with, then we must forego knowledge altogether, because, to begin with, there is nothing else to draw it out of. Verbal difficulty can be avoided by saying that if we know nothing *else* about a hypothesis, then it has a finite probability. (B) "A comunjurum is green," if it means that *all* comunjura are green, is not a good candidate for the principle of indifference, because we know too much about it already, even before "comunjurum" is interpreted to us. We know that there are many colors besides green, and that many things have no color at all; and we know that an A-proposition is equivalent to a *conjunction* of singulars, and so is *a priori* improbable, and that as a matter of fact most A propositions are false. Hence, prior to interpretation, the proposition *The things called 'comunjura' by Mr. Dubs are green* has a probability much less than $\frac{1}{2}$, although finite. On the other hand, the proposition *Square circles are green*, which is what we have to assess after the interpretation, we know to be *a priori* and certainly true (because a self-contradictory property implies any property), just as its contradictory, *There are some square circles which are not green*, is *a priori* certainly false. But the probability of their logical sum is still 1.

Contemporary theory of probability has reached a pitch of refinement where I hesitate to venture any short and decided opinion on a subject so crucial. But if the principle of indifference is to be disposed of, it must be by showing the falsity of one or more of the following propositions, which imply it, and all of which, I must confess, look to me analytic and self-evident. (1) Either p or $\sim p$. (2) If there is equal evidence for p and $\sim p$, they are equally probable. (3) If there is no evidence either for p or for $\sim p$, there is equal evidence for them. But I do not conceive that anything conclusive can be said to this point without being clearer on what is meant by "probability;" until, for instance, we can make a decision between the logical and the statistical views of probability, and can assign their appropriate rights and privileges to such

kindred categories as old-fashioned *probability*, Reichenbach's *weight*, and Carnap's and Popper's *confirmedness*.

Mr. Dubs, I may gratefully remark, is one degree nearer what I believe to be the truth on this subject than Mr. Nelson. Mr. Nelson believes in the external world, but thinks this belief cannot be inductively justified. Mr. Dubs believes in it, and thinks it can be inductively justified, but not by means of the principle of indifference. If Mr. Dubs can show us how Mr. Nelson's objections can be met *without* appeal to the principle of indifference, then the fate of the latter principle will become for both of us a matter of only very abstract and academic concern.

DONALD C. WILLIAMS

University of California at Los Angeles

EXTRA-SENSORY PERCEPTION AND IDEALISM

Dear Sir:

It is unfortunate that the announcement of discoveries or theories seeming to require a radical modification of our views nearly always arouses widespread opposition rather than the careful consideration it should receive. The investigation of extra-sensory perception has brought to light discoveries of this nature, inasmuch as they do not seem, superficially at least, to conform to any of the known facts or laws of science. Undoubtedly it is to the reaction against this seeming innovation that is due the article by H. Rogosin in a recent number of Philosophy of Science (October 1938, p. 472).

Mr. Rogosin seems to be under a few unfounded misapprehensions, however, that perhaps should be pointed out to prevent the casual reader from gaining a wrong impression of the significance of Dr. Rhine's work. He says, for example, "There is no doubt whatsoever as the authors themselves have recognised, that their experimentation is revolutionary, so revolutionary that if upheld, the whole systematic developments of Science up to the present time would be overturned." It is difficult, however, to understand how any of the "systematic developments of science up to the present time" could be overthrown by any possible results of the extra-sensory investigation. They might be greatly broadened, and—in the psychological field—possibly somewhat modified, but overturned—never. Mr. Rogosin will have to be more specific to establish this fact.

Another misapprehension that appears frequently in the article is succinctly put in the introductory synopsis. "The widespread publicity and the consequent furor created by Dr. J. B. Rhine's experiments on telepathy and clairvoyance, have been interpretated in some quarters as a call for a thorough-going revision of the entire field of psychological thought. This revision, it is held, must be in the direction of philosophic idealism, and away from previously accepted materialistic doctrines." Only some basic misconception could make one feel that Dr. Rhine's work was in some way inherently associated with philosophical idealism. The distinction between idealism and materialism or realism is an involved one, and has such a complex historical background that its real nature cannot be explained briefly. Dr. Rhine's, however, is a scientific investigation. He seeks facts as do the physicists in any of their more usual fields; he seeks merely phenomenal facts, in other words, and no discoverable phenomenon (using this term in its original and not its spiritualistic meaning) can affect our decision for or against philosophical idealism.

Moreover, in spite of his many statements and implications that Dr. Rhine and his associates are not proceeding on a strictly scientific basis, he shows little evidence of having the physicist's careful regard for what is justifiably affirmed. He says, for example, "Time and Space existed before mankind arrived on the scene, and will exist even if mankind is wiped off the face of the globe." Any statements he makes of what went on before man appeared or what will happen after he is gone can, of course, be nothing but inference, and no careful scientist would put them in such positive form.

I write this merely to put on record the lack of justifiable basis for his attack—not to refute it formerly.

PHILIP CHAPIN JONES

83 Old Short Hills Road,
Short Hills, N. J.

CONCERNING THE MASS PRODUCTION OF RESEARCH

Dear Sir:

Dr. T. Swann Harding's recent note on "The Mass Production of Research" deserves the serious consideration of thinking men everywhere. He has not exaggerated the gravity of the situation, for science is menaced by a raging torrent of its own creation which grows in volume and intensity with each succeeding year. It has been calculated that

if the present rate of increase of scientists and of scientific publications continues, then before the end of the century most of the inhabitants and resources of this country will be engaged in the preparation and publication of scientific articles. Of course, economic considerations will soon call a halt, but all indications seem to show that scientific publication in the United States has yet to reach its peak.

But the problem that confronts us is far more difficult than apparently Dr. Harding is willing to concede. He seems to recognize two basic aspects of the problem: (1) that of dealing with the current literature, and (2) that of organizing and bringing together material already published. He suggests that the former problem can best be dealt with by cutting down the amount of published material, and that the latter should become the concern of a National Institute of Science. Neither of these solutions, it will be pointed out, is a fundamental or satisfactory answer.

It is true, as Dr. Harding has pointed out, that publishing has become a national mania which is assuming epidemic proportions. It is true that individuals rush into print without proper meditation. It is true that a great amount of scientific work is trivial. No one can thumb through any considerable number of journals without being impressed by the asinine character of large numbers of articles. It is true that the number of publications is not always an index of real progress, and that there is need for emphasis upon quality rather than quantity. There can be no doubt but that a great many publications appear because of academic conditions over which individuals possess little control.

The academic aspect of the problem is perhaps the most tragic, for it affects the nature of research. Topics are selected which will yield quick and publishable results, rather than the long and difficult investigations that are fundamental in so many lines of research. A great deal of worthless research masquerades under the name of pure science, acquiring thereby a sort of intellectual immunity. Now no one will deny that pure science deserves a high and honored place, but it must drive from its protection those who would exploit it for academic ends. We must also forego the superstition that it is somehow more honorable to be impractical. If some of our difficulties arise out of academic conditions, then why not deal with the problem at its source? If the avenues of publication are limited without modifying the academic conditions which drive individuals to publication, we will simply increase the amount of scientific frustration.

Dr. Harding favors the elimination of "trash" papers which make no

fundamental contributions. But who is competent to judge whether a paper is valuable or not? Where the work reported seems well done, what justifiable reason can there be for denying it publication? Petty details often hold the keys to great inventions and far-reaching discoveries. What may seem trivial to one man may be the most important thing in the world to another. Not every paper can be a fundamental contribution, and science has grown strong on odds and ends.

In quoting the Bishop of Ripon and Sir Ernest Rutherford in behalf of a scientific holiday during which no scientific advances of consequence would occur, does not Dr. Harding concede that the real difficulty is that we cannot keep pace with the important advances? If only a tenth of the material in the world's 36,000 scientific journals were deserving of publication, we would still be flooded by a tremendous and unmanageable literature. To declare a moratorium on ideas is to run away from the problem rather than to meet it squarely.

In his proposed National Institute of Science Dr. Harding seeks to deal with the knowledge that has already been accumulated. Such an institution would be a step in the right direction, but it could hardly solve the problem.

Scientific facts are as vital to scientific investigation as test tubes or retorts; at our disposal today lie thousands of new instruments and millions of facts which make possible researches undreamed of a generation ago. To resurrect the vast literature that lies entombed in library vaults everywhere is a gigantic and difficult undertaking. Dr. Harding states that his Institute would tell the exact status of a given problem the world over, relate it to other problems, determine whether methods and equipment exist with which to attack it effectively, and act as a clearing house for information of all kinds. Such an Institute would be the answer to every scientist's dreams. What worker in science has not experienced the feeling that buried somewhere in the literature was material bearing on his work, facts which would clarify his task and increase his ability to deal effectively with it? Research has not operated at its maximum efficiency because a great deal more relevant material is available than has been employed.

But is such an Institute possible? The ability to organize and collect relevant facts is not easily duplicated by a clerical staff, however, qualified. Indexing services, for instance, suffer from the inability to grasp relationships. By definition, genius is the ability to see through apparently unconnected facts and events, though once diagnosed, the train of facts which they entail may be excavated by routine procedures.

Problems overleap the artificial boundaries of categories, and those disciplines which seek to translate the findings of the laboratory to the needs of everyday living—such as the applied sciences of medicine and engineering—avail themselves of human experience at any level, and are compelled to employ the findings of many sciences. A fact cannot be placed into one neat pile or category. The same chemical fact may have entirely different connotations for the physician, chemist, psychologist, and physiologist. Furthermore, these connotations cannot always be predicted *a priori*. Does Dr. Harding propose to manufacture insight at his Institute? If he does, he will have to staff it with geniuses.

One of our great difficulties is that we have not placed sufficient emphasis upon the connected nature of knowledge. Such interconnections arise not only from fortunate insights, but, above all, from carefully planned investigations designed to determine relationships. We need more investigations of this sort if we are to effectively organize the facts we have.

As realists, we must recognize that it is impossible to stop the output of scientific articles save by the severest and most extraordinary measures; that the probability is that publications will increase rather than decrease. Our only hope is to accept it as inevitable and attempt to harness it to the purposes of science. It must be confessed that there exists, as yet, no specific for publicationitis.

Oscar Kaplan

University of California, Berkeley

Reviews and Notes

A General Schema for Natural Systems (Nature Considered as a Function of Types of Selectivity and of Modes of Selection). By Irwin Biser. 155 pages. Westbrook Publishing Co., Phila.

This is an ambitious first draft of a synthesis from study notes in the philosophy of science. Dr. Biser is fresh from "Kant seminars". His chief school influences are his teachers, Edgar A. Singer, Jr. and William Marias Malisoff. The former has modified idealism by a thoroughgoing experimentalism, the latter has modernized materialism by inclusion of dynamic physical theory and logical research. Dr. Biser seems to change his stress on these factors as suits his style and development.

The style is a la Wittgenstein in the "Tractatus", with numerous worthwhile inclusions of illustrations from physics. The author's key synthetic sentence is: "Nature is the totality of things in the qualitative-quantitative sense to be apprehended by a subject by the application of selected concepts to sense-data". The rest of the book is elucidation of this in a seemingly deductive manner. The features of the world are exhibited in a *conceptuo-sensuous manifold*, designated (C-S). "The subject constantly exercises (C-S) selection". Selection stands for all sorts of cognitive apprehension. An amoeba, for instance, sees the world and "selects" from an "amorphous content" all that its biological structure enables it to "apprehend" and only that. Conceptual processes are not to be taken as totally divorced from sensuous processes, which explains the hyphen in "conceptuo-sensuous". Logical structures are included in the "selection". Subjects show "creativity" in choosing conceptual patterns, which results in types of selectivity, "possible perspectives" and "nature-perspectives". For details see the original.

Dr. Biser should be encouraged.

W. M. M.

Toward a Dimensional Realism. By Charles M. Perry. Norman: The University of Oklahoma Press. 1939. viii + 180 pp. $2.50.

The Dimensional theory as Dr. Perry presents it—impossible to summarize in brief space—is interesting and fertile. But unfortunately not sufficient attention is given to the various meanings of the term "dimension" as used in current theories (for example, Boring, Hartshorne, Korzybski, Dunne, the configuration space of wave mechanics, dimensionality in relativity theory, the life space of Lewin's topology, etc.), nor to the criticisms of this concept as an *explanatory* notion (e.g., James MacKaye's analysis of Einstein's dimensional explanation as surrendering the *dynamic* feature of nature). Nevertheless this is a significant contribution to an important line of thought.

Oliver L. Reiser

University of Pittsburgh.

A Preview of the Absolute-Relative Theory. By Franklin J. Matchette. 26 pages. Published by the author, 817 Fifth Ave., New York. 50¢.

Attempts at unification are very infrequent these days. The author presents a synopsis, which is so compact as to warrant no further condensation, of a thoroughly dualistic philosophy. The dualism, however, is not of matter and mind, but of transient, contingent and dependent Becoming and eternal, necessary and self-subsistent Being. Charts bring out the author's care in laying out the build-up of the relative world so that there is enough order in it to serve as a guarantee for *science.* The bricks of Mr. Matchette's world are called "zero-atom-units". All structures therefrom show "polarity" and "divergence" from the absolute. Every effort is made to fit modern science. It will be interesting to see how that is done in detail in a large volume to come.

W. M. M.

"Truth" as Conceived by Those Who are not Professional Philosophers. By Arne Ness. 178 pages. I. Kommisjon Hos Jacob Dybwad, Oslo. Kr. 12,00.

This may be described as a psychological study. The author has invoked the paraphernalia of questionnaires and statistical analysis. Amateurs, such as young school boys and girls, gave practically the same spectrum of replies as professional philosophers, the differences

being those of degree. "Opinions on philosophical and metaphysical subjects (e.g. truth theories) can be conceived as retained pubertic formulations remodelled and deepened under the influence of formulations transmitted by tradition". High-hat philosophers have a bad time in this sparkling book.

W. M. M.

Die Enstehung des Lebens durch Stetige Schöpfung. By Ignaz Lichtig. N. V. Noord-Hollandsche Uitgevers Maatschappij, Amsterdam.

The focal thesis of this book is a defence of the view that the primordial generation of life was a continuous process along numerous "Abstammungslinien" independent of each other. The evidence adduced from morphology, paleontology etc. against the theory of generation from one primordial cell is ample and plausible. The author holds that the conditions making for life are omnipresent in "abiotic" matter at all times. It is regrettable to find reference to Cantor's theory of irrationals as strengthening the notion of continuity of the generation of life (p. 13). The book represents a monument to effort and is commendable for its bibliography and appendices.

IRWIN BISER

Philadelphia, Pa.

The Old Faiths Perish. By Jay William Hudson. 302 pages. D. Appleton-Century Co., New York. $2.00.

The minimum requirement for a religion is to be reasonable and to furnish "salvation". A "lazy-minded solution" postulates two truths, the scientific and the religious; a "narrow-minded" solution tolerates only the scientific method; a "sceptical-minded" solution is the agnostic one. The author dislikes all the three. What men seek is total, rational and social self-realization. Out of such a program the author wishes to build a religion to replace the old faiths which perish. Like all religions, however, it must have a faith above logic. The book ends rather abruptly with an appeal to such a faith.

W. M. M.

Who Was Socrates? By Alban D. Winspear and Tom Silverberg. 96 pages. The Cordon Company, New York. $1.25.

Scholars will wrangle over this book. The authors are pioneering the "genetic method", which represents the *evolution* of a man's thought, in the case of Socrates. A large task, well utilized by the authors,

has been to trace the relation of Socrates to the political struggles and social currents of his time. A continuity had to be established between the Socrates of the *Apology* and the Socrates of the *Clouds*. A very human Socrates emerges, perhaps all too human.

W. M. M.

Perpetual Peace. By Immanuel Kant. 67 pages. Columbia University Press. $1.00.

It is remarkable how timely this essay of 1795 is even to-day. A foreword by Dr. Nicholas Murray Butler emphasizes this point.

W. M. M.

Formal Logic. A Modern Introduction. By Albert A. Bennett and Charles A. Baylis. 407 pages. Prentice-Hall, Inc., New York. $2.75.

"The study of logic is an objective and impartial search for those principles which are sufficient to validate reasoning, an examination of them in their interrelations, and an attempt to make them explicit in well-organized systems". It is a "rational science". Most of this volume is devoted to bringing up-to-date the material for good texts in formal logic. The last of 13 chapters is devoted to "scientific method" and is necessarily very brief. There are numerous interesting exercises and references for further study. The style is intriguing and it is apparent from the frequent bursts of opinion that not all is settled in logic,—an excellent thing for students.

W. M. M.

The Principles of Statistical Mechanics. By Richard C. Tolman. Oxford University Press, 1938. pp. 660. $9.00.

This book contains an extensive study of the principles of statistical mechanics in modern form, clearly presented with great care for the fundamental aspects. Its aim is not to give applications but rather to explain the underlying ideas.

The first part is devoted to classical statistical mechanics; the second part to statistical quantum mechanics; and finally the third part deals with thermodynamic applications. The reader, even without any previous knowledge of mechanics and quantum mechanics is led to a deep understanding of statistical methods in physics. The book can be recommended strongly to all physicists and philosophers interested in the basic physical problems.

L. Infeld

The Concepts of the Calculus. By Carl B. Boyer. 346 pages. Columbia University Press, New York. $3.75.

Those who are familiar with elementary calculus and want to make sure that they have the "correct" view of the foundations, which have been frequently confused in the past, should put this book on the "must" list. The treatment is historical, the emphasis is on concepts.

W. M. M.

The Scientist in Action. A Scientific Study of His Methods. By William H. George. 354 pages. Emerson Books, Inc., New York. $3.00.

"I wish to emaphsize that in reaching the conclusion that some type of action should or should not be done, or is either moral or immoral, he (the scientist) is unscientific". "The second of the limitations of scientific method is the abandonment in the practice of scientific research of assessment of value". The author, you see, is a complete isolationist and a non-interventionist. The reviewer suggests that the book be re-titled, "The Scientist in Reaction".

W. M. M.

Crystalline Enzymes. The Chemistry of Pepsin, Trypsin and Bacteriophage. By John H. Northrop. 176 pages. Columbia University Press, New York. $3.00.

The crystalline proteins and the sober work on bacteriophage ought to concern closely those philosophers who still want to know just how the non-living can become the living.

W. M. M.

Chemistry in Relation to Biology and Medicine with Especial Reference to Insulin and Other Hormones. By John Jacob Abel. 79 pages. The Williams and Wilkins Co., Baltimore, Md.

A splendid account of scientific pioneering by a pioneer. Beautiful printing makes this edition, limited to 825 copies, very valuable.

W. M. M.

BOOKS RECEIVED

Reason. University California Publications in Philosophy. Volume 21. 228 pages. University of California Press, Berkely, Calif. $2.25.

International Encyclopedia of Unified Science. University of Chicago Press. $1 each.

Vol. 1, No. 3. Foundations of Logic and Mathematics. By Rudolf Carnap.

Vol. 1, No. 4. Linguistic Aspects of Science. By Leonard Bloomfield.

Vol. 1, No. 6. Principles of the Theory of Probability. By Ernest Nagel.

The Kantian Philosophy of Space. By C. B. Garnett, Jr. 287 pages. Columbia University Press. $3.50.

Psychologie. Wesen und Wirklichkeitder Seele. By Maximilian Beck. 271 pages. A. W. Sijthoff's Uitgeversmoatschappij N. V. Unbound $6.50.

A History of Science, Technology and Philosophy in the Eighteenth Century. By A. Wolf. 814 pages with 345 illustrations. The Macmillan Co., New York. $8.00.

Knowledge for What? (The Place of Social Science in American Culture). By Robert S. Lynd. 268 pages. Princeton University Press. $2.50.

The Social Function of Science. By J. D. Bernal. 482 pages. The Macmillan Co., New York. $3.50.

Problems of Ageing. Edited by E. V. Cowdry. 758 pages. The Williams & Wilkins Co., Baltimore, Md. $10.

The Place of Value in a World of Facts. By Wolfgang Köhler. 418 pages. Liveright Publishing Corporation, New York. $3.75.

ANNOUNCEMENT

The Fifth International Congress for the Unity of Science will be held at Harvard University, September 3–9, 1939.

Rooms will be available at $1.25 per day per person. Arrangements can be made through Dr. W. V. Quine, Harvard University, Cambridge, Mass.

Contributions to the Congress are as follows: Donors, $25.00 upwards; active members, $5.00; students and relatives of members, $1.50. Donors and active members receive a copy of the proceedings. American participants may make their contributions payable to the order of the Fifth International Congress for the Unity of Science, University State Bank, 1354 East 55th Street, Chicago, Illinois.

All others who would like later notices of the Congress should send their name to Professor Charles W. Morris, University of Chicago, Chicago, Illinois.